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THE EFFECT OF LOCAL ANESTHESIA ON TACTILE AND VIBRATORY THRESHOLDS [1]

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In spite of the progress that has been made in recent years in analysing the phenomena of vibratory sensitivity (pallesthesia) no general agreement has been reached concerning its peripheral mechanism. Theories have been advanced that the vibratory sensation is a quality of superficial touch, a quality of deep sensitivity and even the manifestation of a separate and autonomous sensory modality.

The present experiment was designed to submit these conflicting views to a direct test by determining the effect of local tactual anesthesia on the absolute threshold of vibration. It seems evident that if tactile end-organs are involved in the vibratory sensation, their anesthetization should raise the vibratory threshold. Furthermore, this change should vary with the number of touch receptors anesthetized. The latter expectation is a necessary consequence both of the physical conditions of stimulation and the response characteristics of the possible receptive mechanisms. Since the physical vibrations are not confined to the site of stimulation but spread over a more or less extensive area, the stimulating conditions invariably provide for the excitation of a number of end-organs. The excitation of a considerable number of receptors is also essential in order that the wide variations of intensity and the high frequencies (2000–3000 ~) which still give rise to vibratory experience can be adequately repre-

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sented in the peripheral nerve message. Recent physiological investigations by Adrian, Cattell and Hoagland (1) and Cattell and Hoagland (4) have indicated that the maximal discharge rate of single end-organs of touch and deep pressure is of the order of 200–300 impulses per second. In the case of touch this rate cannot be maintained for more than a few seconds. In addition, the all-or-none property and the refractory phase of nerve response further limit the effectiveness of a single fiber in representing the frequency and intensity characteristics of the physical stimulus. Therefore, the representation of these variables can only occur as a result of the simultaneous action of a number of receptors. Now if the response of a large number of essential end-organs is abolished by the anesthetization of a large area of the skin, the effect on sensitivity should be greater than if a small area (and few end-organs) had been affected. If no change in vibratory sensitivity resulted from tactual anesthesia, it would be clear that superficial touch is not involved in the vibratory experience.

Several investigations of the effects of local anesthesia on tactile and vibratory sensitivity have been reported in the literature. The results of these experiments are conflicting and impossible to reconcile since identical techniques were employed. Goldscheider (18) reported that even after the light, superficial anesthesia obtained by freezing methods "one can convince oneself that the vibrations are less intense on this area." After subcutaneous cocain injections the vibrations "are very poorly or not at all felt." He added, however, that if the pressure of the stimulating fork was increased sufficiently, the vibrations could be perceived at the cocainized area. On the other hand, Bing (2) and Cohen and Lindley (5) have reported that the intensity of the vibratory sensation was not appreciably weakened after cocain or novocain infiltration. With ethyl chloride spray or carbon dioxide snow vibratory sensitivity was actually increased. This result has been ascribed to an increase in the density of the tissues produced by freezing.

Procedure

The procedure of the present experiment consisted in determining the absolute intensive thresholds for touch and vibration before and after cutaneous anesthetization. This procedure required apparatus of sufficient delicacy to give accurate quantitative measurements of light pressures and vibrations at threshold intensities and of sufficient range to detect any change of sensitivity that might result from the anesthesia. It also required a method which provided a complete and lasting anesthesia.

Method of Anesthesia.—The method of anesthesia consisted in the introduction of an alcoholic solution of cocain into the skin of the inner surface of the forearm by means of an electric potential applied to the skin surface. Cocain, which readily penetrates mucous membrane, can be made to pass across the skin membrane only when a potential is employed. The biochemistry of this process is not fully understood but it is thought to involve not so much an infiltration of cocain ions as an introduction of the non-electrolytic solution of cocain as such, as a result of osmotic conditions set up at the surface of the skin by the current. This process is termed electroendosmosis. The perfection of the endosmotic method of anesthesia for surgical use has recently been reported by the German physiologist Rein (**24, 25, 26**). The apparatus and procedure recommended by him were employed in the present experiment with only minor changes.

The apparatus required only a simple circuit consisting of a battery, a milliammeter for measuring the current strength, a variable resistance for regulating the flow of current, an active electrode of thin silver foil and a 1 percent solution of sodium chloride which served as the indifferent electrode. The anesthetic was a 2 percent solution of the free base of cocain in 80 percent alcohol.

The subject rested the extensor (outer) surface of his forearm in a half inch bath of the sodium chloride solution. A square pad of ash-free filter paper soaked in the anesthetic solution was placed on the opposite surface of the arm directly over the area of the skin to be anesthetized. The active electrode, which was of the same size and shape as the filter paper pad, was adjusted to the pad and held firmly in place by means of adhesive tape. A weak current (.3 to .5 milliampere for each square centimeter of active electrode) was then turned on and allowed to flow for a period of 8 to 10 minutes. During the flow of current the filter paper pad was kept moist by occasionally adding a few drops of the cocain solution from a fine pipette.

In order to increase and prolong the anesthetic effect a small quantity of commercial adrenalin was later added to the solution to constrict the capillaries and prevent the anesthetic from being carried away in the blood stream. By varying the size of the active electrode (and the underlying pad), the size of the area affected could be easily and accurately controlled. Areas of two different sizes were anesthetized in the present experiment.

The current was increased and decreased very slowly to avoid unduly unpleasant sensations. Usually the subjects reported a slight prickling or burning sensation during the first few minutes of current flow.

For the purposes of the present experiment, the endosmotic method possesses a number of advantages over the more common types of local anesthesia. It is superior to freezing with respect to the duration and degree of the anesthetic effect. It is superior to subcutaneous injection and nerve block in that there is a more accurate control of the number of receptors affected and less likelihood of reaching deep receptors. At present, there is no direct means of determining precisely how far the cocain penetrates. However, indirect tests by von Frey, Rein and Strughold (**14**) indicate that

after an anesthetization of 8–10 minutes the effect is restricted to the epidermis and cutis. While the entire thickness of the skin could be pierced without pain, the underlying tissues were still sensitive. The preservation of local proprioceptive reflexes under the anesthetized area also indicated that the receptors and nerves in the muscles and tendons were unaffected. In addition, Hausmann (19) has reported that pressure sensitivity in the deeper tissues does not disappear until after a 60 minute infiltration of the anesthetic solution.

Apparatus for Determining the Tactual Thresholds.—Tactile sensitivity was measured by means of spring limen gauges similar to the instrument described by Boring (3). The gauge consisted of a brass cylinder (150 × 13 mm.) in which a projecting rod was supported by a delicate spring. One end of the spring was attached to the rod; the other end, which passed through a longitudinal slit in the tube, was attached to an external metal ring which could be freely moved up and down the tube. The movement of the rod was guided with a minimum of friction by a bevelled opening in a plug attached to the lower end of the cylinder.

When the rod was set upon a resistant object, a downward pull of the ring increased the tension on the spring. As a consequence, pressure was exerted on the object. A calibrated scale for indicating pressures was attached to the cylinder. The pressures exerted by the gauges were calibrated in terms of 'grams per millimeter of radius of the stimulating surface.' This is the so-called tension unit which von Frey (13) found to be most satisfactory when dealing with small stimulating areas.

Two limen gauges were employed in the present experiment. One, for use before anesthesis, yielded pressures from 0 to 5 grams per mm. of radius in quarter gram steps. A second gauge, for use after anesthesis, yielded pressures from 0 to 18 grams per mm. of radius in half gram steps. The stimulating surface of the rods in both instruments was 1 mm. in diameter. The gauges were mounted on universal standards and could be readily adjusted at the precise point of stimulation.

Apparatus for Determining the Vibratory Thresholds.—The accurate measurement of vibratory sensitivity requires apparatus which is capable of producing vibrations of pure wave form and of providing minute variations in amplitude. These requirements have been successfully fulfilled by a proper adaptation of modern sound producing apparatus. The apparatus used in the present experiment was essentially similar to the systems employed in most of the recent investigations of vibratory sensitivity (16, 23).

Vibrations were produced in a magnetic loud speaker unit (Duo Magnetic Unit) to the armature of which was attached a thin bakelite rod which served as a stimulating point. The speaker motor was driven by a system consisting of (*a*) a 2-A audiometer which was set to generate a frequency of 256~, (*b*) an attenuator (Western Electric D 77606) which was calibrated to provide variations of intensity in steps of 1 decibel, (*c*) an amplifier (Lafayette 2A3) which increased the current input of the speaker motor sufficiently to provide amplitudes of vibration that were perceptible on the skin (*d*) a step down transformer which was inserted between the audiometer and the attenuator to secure the purest possible wave form by matching the impedances of these units and (*e*) a high pass filter which was inserted between the amplifier and the speaker unit to eliminate an alternating current hum of the amplifier.

The speaker unit which served as the vibrator was mounted on a universal standard. Since the tone generated in the system was audible at the vibratory threshold an electric fan was used to provide a masking tone.

A measure of vibratory sensitivity was obtained in terms of the number of decibels that an arbitrary current strength was attenuated at threshold. This value could be

read directly from the attenuator dial and provided an adequate index for a comparison of vibratory sensitivity before and after anesthesis. The greater the attenuation, the smaller was the amplitude of the physical vibrations. Therefore, relatively high sensitivity was represented by a large number of decibels of attenuation and decreased sensitivity by a smaller number of decibels.

Procedure.—Six adult male subjects, experienced in psychological observation, participated in the experiment. Threshold determinations in all cases were made on the flexor (inner) surface of the right forearm. Five adjacent touch spots at the base of hairs were located under a binocular microscope and marked with a spot of India ink. The area was then shaved and inspected under the microscope to make certain that the hair had been completely removed. The particular areas of the arm which were investigated varied somewhat with individual subjects depending on the distribution of the hair. The site selected was always as near the mid-line as possible and at least 10 centimeters from the elbow or wrist. In a series of preliminary experiments different areas within this region were investigated. In the final series of experiments the same area was tested in four successive experimental sessions.

The subject placed his arm in a comfortable position in a specially constructed plaster cast which was attached to the arm of a chair. With the arm in a horizontal position, the stimulating rod of the limen gauge was placed directly over one of the marked touch spots. After a 'Ready' signal from the experimenter, pressure was gradually applied to the spot. Care was taken in each trial to increase the pressure at as uniform a rate as possible. The subject responded as soon as he was aware of a tactile sensation. Because of the rapid adaptation of touch, only an ascending order of stimulation was used. Five successive measurements were made on each of the five spots. Since the different spots gave very similar results, an average of the twenty-five measurements was computed to give an index of the tactile sensitivity of the area.

The same five spots and a similar procedure were used in determining the vibratory thresholds except that both an ascending and descending order of stimulation were employed and fifty measurements were made in each experimental session. Again, the mean of the measurements was computed to obtain an index of the vibratory sensitivity of the area. Since the size of the stimulating surface and the pressure exerted by the vibrating rod had been found in exploratory experiments to affect the threshold values, these factors were kept as constant as possible from trial to trial. This was accomplished by slowly lowering the blunted point of the stimulating rod until an indentation of the skin was just visible. The area of the rod which was brought in contact with the skin by this procedure was approximately one millimeter in diameter.

After normal tactile and vibratory sensitivity had been determined, the area was anesthetized. Immediately following anesthesis, the same spots were investigated for touch and vibration according to an identical procedure. The same procedure was followed in each experimental session except that the order of determining tactual and vibratory thresholds was reversed in successive sessions.

In all the experiments of a preliminary series and in half the experiments of the final series an area of 4 square centimeters was anesthetized. In the remaining experiments of the final series an area of 9 square centimeters was anesthetized. The 4 square centimeter area was selected because it was always sufficiently large to provide a wide border of anesthetized skin around the touch spots which were investigated. Such a border was necessary in order to prevent the stimulation of touch receptors beyond the anesthetized area. The 9 square centimeter area was used to determine the relationship between the size of the anesthetized area and amount of change in sensitivity.

Results

Preliminary Experiments

1. *Effect of Partial Anesthesia.*—In the first series of preliminary experiments no adrenalin was added to the anesthetic solution. The series consisted of five experiments performed on two subjects. One subject served in three experiments, the second in two experiments. Different areas of the arm were anesthetized in each experimental session.

Without the addition of adrenalin to the solution, only partial tactual anesthesia was obtained. While tactile sensitivity was somewhat decreased, the receptors were still capable of responding to increased pressure. The mean threshold values obtained before anesthesis varied from .86 to 1.33 grams per mm of radius.[2] After anesthesis the thresholds varied from 3.56 to 6.56 grams. The loss in sensitivity was consistently greater in the case of the second subject.

The effect of this degree of tactual anesthesia on vibratory sensitivity is shown in Table I. Column 1 indicates the name of the subject. Column 2 shows the mean of 50 measurements obtained before anesthesis; Column 3 the mean of 50 measurements after anesthesis. Column 4

TABLE I

Effect of Partial Anesthesia on Vibratory Thresholds

4 sq. cm. Areas

Subject	Thresholds in db *		Difference in db
	Before	After	
Wi.	19.3	19.1	.2
	17.9	18.4	−.5
	19.3	19.0	.3
In.	17.2	15.6	1.6
	17.4	13.6	2.8

* Each value is the average of 50 measurements.

[2] These values are of the same order as those obtained by Kiesow (22) who reported a mean of 1.24 grams per mm. of radius and a mode of 1.0 grams for similar regions of the arm.

shows the difference in sensitivity in decibel units. The larger this value, the greater is the decrease in sensitivity.

Table I shows that there was no significant change in vibratory sensitivity in three experiments with Subject Wi. and that there was a loss of vibratory sensitivity of less than 3 decibels in two experiments with Subject In. The decrease in sensitivity in the latter case corresponds with the previously mentioned greater decrease in sensitivity to touch. The experiments with this subject served to show that a slight alteration in vibratory sensitivity could be obtained even under conditions of partial tactual anesthesia.

The reason for the difference in the results obtained with the two subjects was not discovered. This difference may have been due to the degree of anesthesia which was produced or to the particular regions selected for investigation. The possibility that individual idiosyncracies in reporting affected the results was eliminated by the fact that a decrease in vibratory sensitivity was obtained with both subjects in a later series of experiments.

2. *Effect of Complete Anesthesia.*—In order to obtain a complete tactual anesthesia, in the remaining experiments one drop of adrenalin was added to each cubic centimeter of the anesthetic solution. The adrenalin caused a decided blanching of the tissues that gradually disappeared with the return of normal circulation. Within approximately an hour, the appearance of the skin was again normal. The return of sensitivity was also gradual. Although complete tests on the duration of anesthesia were not made, no change in the anesthetic effect was found for at least 25–30 minutes.

In the first series of experiments in which adrenalin was used, different areas of the arm were investigated in each experimental session. Four subjects took part in these experiments. One of these subjects, In., had served in the preceding series.

With the addition of adrenalin, tactual sensitivity was abolished in the anesthetized area. Before anesthesia the mean threshold values varied from .59 to 1.77 grams. During anesthesia no response was obtained to pressures as great as

18 grams (the maximum of the heavier gauge). Complete tactual anesthesia was obtained with all subjects.

The changes in vibratory sensitivity obtained in the ten experiments of this series are shown in Table II, Column 4.

TABLE II

EFFECT OF TOTAL ANESTHESIA ON VIBRATORY THRESHOLDS

4 sq. cm. Areas

Subject	Thresholds in db *		Difference in db
	Before	After	
Ka.	18.3	17.2	1.1
	19.2	14.9	4.3
	16.7	8.7	8.0
Gu.	20.7	21.7	−1.0
	18.5	9.9	8.6
	20.4	9.8	10.6
Hu.	19.4	18.8	.6
	21.7	18.6	3.1
In.	25.8	23.0	2.8
	18.9	14.6	4.3

* Each value is the average of 50 measurements.

It may be seen from Table II that a decrease in vibratory sensitivity of more than one decibel was obtained in seven of the ten experiments of this series. In this group of experiments, every subject showed a decrease in sensitivity in at least one experiment. In comparison to the total loss of tactual sensitivity obtained in the same regions of the arm, the changes which occurred in vibratory sensitivity were small. The maximum loss which was obtained in any experiment amounted to 10.6 decibels. The differences which were found in the various regions of the arm did not appear to be related to the degree of sensitivity of the regions, as a comparison of Columns 2 and 4 shows.

Table II also shows that there were areas in which no significant change in vibratory sensitivity occurred in spite of complete tactual anesthesia. This situation was found to occur in certain regions of the arm where the vibrations were localized, not at the site of stimulation, but at a remote point,

near the elbow or wrist. In every area in which this anomoly of localization was reported, no reliable change in vibratory sensitivity was obtained after anesthetization at the site of stimulation. This peculiarity of localization, which was spontaneously reported by three subjects, was probably due to placement of the stimulating rod directly over a tendon which conveyed the vibrations to the rich sensory field of a joint.

The experiments of this series suggest two reasons why a tactual component of vibratory sensitivity may not have been detected in previous investigations of cutaneous anesthesia occurring pathologically or resulting artificially from anesthesis. First, the alterations in vibratory sensitivity are often so slight that they can only be demonstrated by means of very delicate instruments. With the tuning fork techniques commonly used in the past, these small changes could easily have escaped detection. Second, certain areas of the body may be found in which no apparent alteration of vibratory sensitivity accompanies tactual anesthesia. These are most probably areas where the underlying tissues and structures provide for an efficient transmission of the stimulus to more sensitive regions beyond the affected part. Under these conditions, the subject is likely to report on the sensitivity of a region other than the one under investigation. Therefore the absence of an effect in a given region does not necessarily demonstrate that cutaneous receptors are not involved in vibratory sensitivity. This was clearly demonstrated even within the limited region of the forearm explored in the present experiment.

Final Experiments

In the final series of experiments, a single area of the arm was investigated in four successive experimental sessions. The same five spots within this area were investigated in every session. In two of the experiments, an area of 4 square centimeters was anesthetized and in the remaining two an area of 9 square centimeters was anesthetized. Four subjects took part in this series. Subject In. had served in the two preliminary series, Subject Wi. in the first preliminary series

and Subject Ka. in the second preliminary series. Subject We. had not previously been used. In all the experiments adrenalin was added to the cocain solution.

Before anesthesis, the mean tactual threshold values varied from .86 to 1.70 grams per mm. of radius. After anesthesis, tactual sensitivity was abolished in all experiments except one. In this experiment a single touch spot was found to be sensitive to pressures of about 10 grams. Certain spots were consistently described by the subjects as 'bright,' 'ticklish,' 'lively'; others as 'vague,' 'diffuse.' These qualitative differences did not appear to be related to differences in the relative sensitivity of the individual spots. No evidence for gradations of intensity was obtained with single touch spots.

The changes in vibratory sensitivity which were obtained in this series of experiments are shown in Figs. 1-*a* to 4-*b*. Each figure shows the frequency distribution of the threshold values obtained before and after anesthesis for an individual subject. Two figures are presented for each subject. The results obtained in the two experiments with the 4 sq. cm. area were combined and are shown in the figure on the left; the results of the two experiments with the 9 sq. cm. area are shown in the figure on the right. In all figures, the abcissa shows the threshold values in terms of the number of decibels of attentuation of an arbitrary current strength; the larger this number, the greater the sensitivity. The ordinate shows the number of times that a given value of the abcissa was obtained. The solid line represents the frequency distribution of 100 judgments obtained before anesthesis; the broken line, the frequency distribution of 100 judgments after anesthesis.

As may be seen from these figures, a small but consistent decrease in vibratory sensitivity was obtained after cutaneous anesthesis. It is also evident from the figures that the change in sensitivity was not significantly greater after anesthetization of the larger area.

The same results are also shown in Table III. Column 1 indicates the name of the subject. Column 2 shows the

mean of 100 threshold measurements obtained before anesthesis of the 4 sq. cm. area; Column 3, the mean of 100 measurements after anesthesis. Column 4 shows the difference in sensitivity in decibels and Column 5, the significance

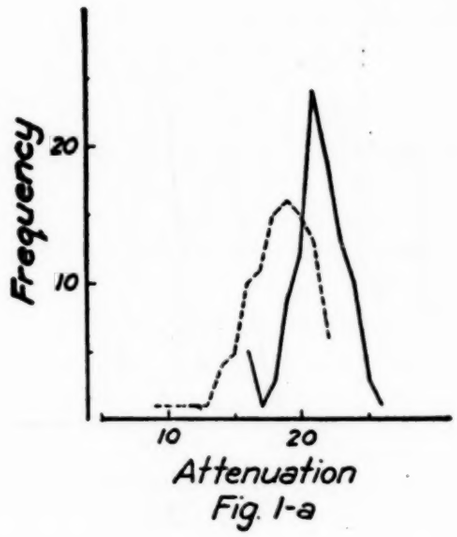


Fig. 1-a

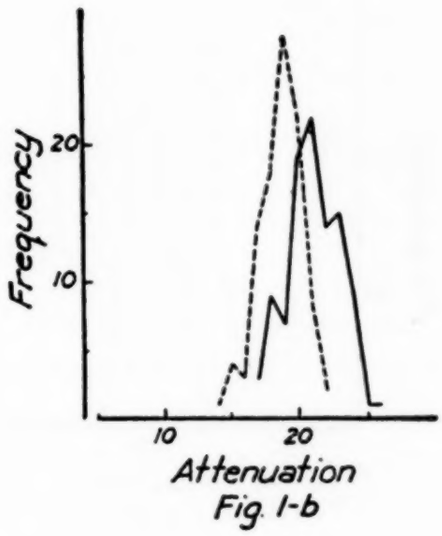


Fig. 1-b

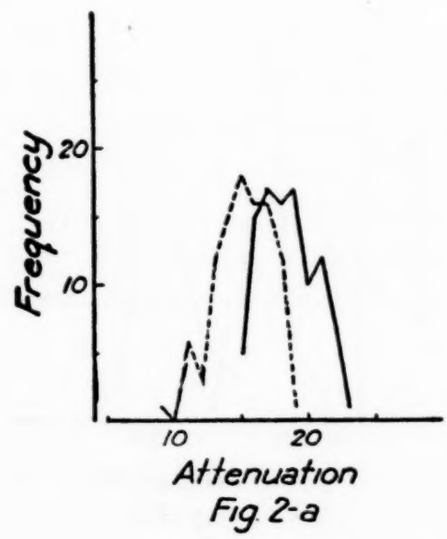


Fig. 2-a

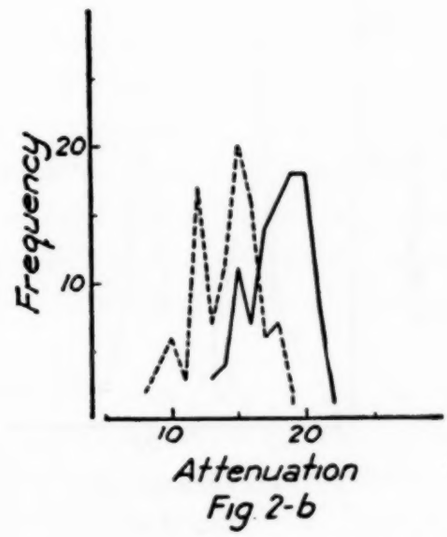


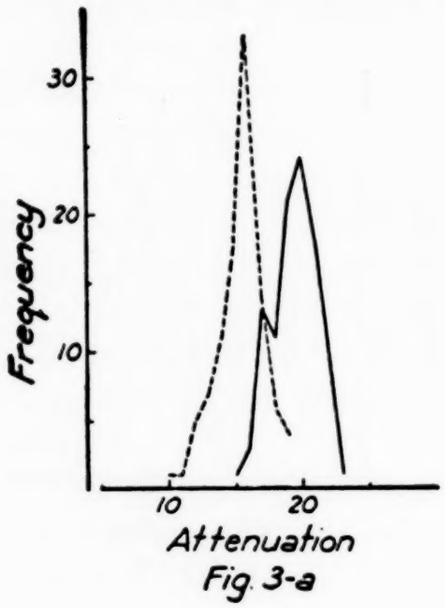
Fig. 2-b

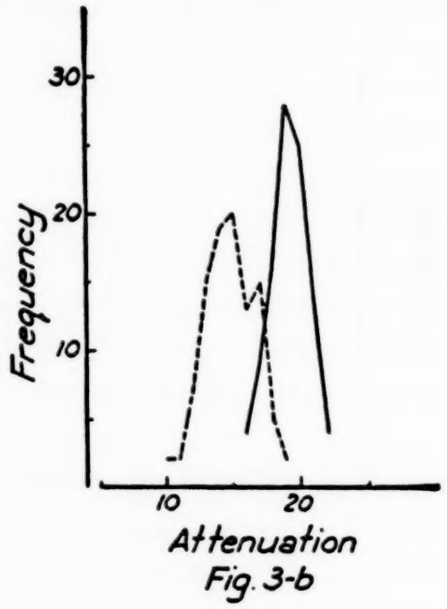
FIGS. 1-*a* to 2-*b*. Frequency distribution of 100 vibratory threshold judgments before anesthesis (solid line) and 100 judgments after anesthesis (broken line). Abscissa represents decibels of attenuation; ordinate, frequency of judgments.

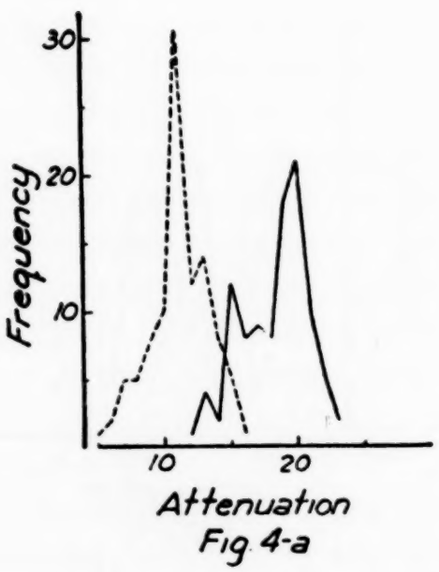
FIG. 1-*a*. Subject We. 4 sq. cm. area.
FIG. 1-*b*. Subject We. 9 sq. cm. area.
FIG. 2-*a*. Subject Ka. 4 sq. cm. area.
FIG. 2-*b*. Subject Ka. 9 sq. cm. area.

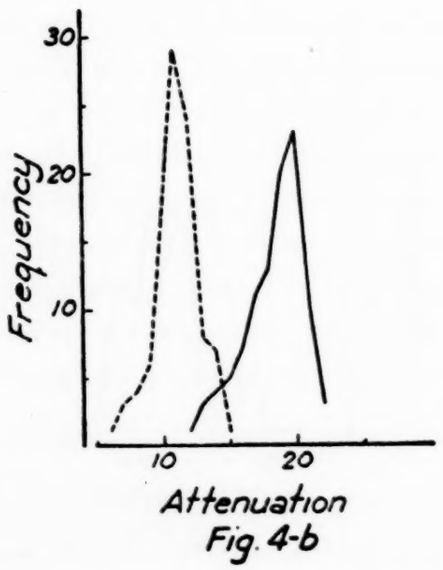
of this difference in terms of χ^2s. Columns 6, 7, 8 and 9 show comparable values for the 9 sq. cm. area.

It may be seen from Table III that a small decrease in









FIGS. 3-*a* to 4-*b*. Frequency distribution of 100 vibratory threshold judgments before anesthesis (solid line) and 100 judgments after anesthesis (broken line). Abscissa represents decibels of attenuation; ordinate, frequency of judgments.

FIG. 3-*a*. Subject In. 4 sq. cm. area.
FIG. 3-*b*. Subject In. 9 sq. cm. area.
FIG. 4-*a*. Subject Wi. 4 sq. cm. area.
FIG. 4-*b*. Subject Wi. 9 sq. cm. area.

TABLE III

EFFECT OF SIZE OF ANESTHETIZED AREA ON VIBRATORY THRESHOLDS

Subject	4 sq. cm. Area				9 sq. cm. Area			
	Thresholds *		Diff. in db	χ^2	Thresholds *		Diff. in db	χ^2
	Before	After			Before	After		
We.....	21.2	18.1	3.1	47.2	21.1	18.7	2.4	48.8
Ka.....	18.4	15.1	3.3	52.4	17.9	14.0	3.9	61.9
In.....	19.4	15.5	3.9	108.0	19.2	14.6	4.6	113.1
Wi.....	18.3	11.2	7.1	133.2	18.3	11.1	7.2	140.9

* Each value is the average of 100 measurements.

vibratory sensitivity was obtained in each set of experiments. As in the preceding series, the change in pallesthetic sensitivity was found to be very slight in comparison to the marked effect on touch. The differences in sensitivity resulting from cutaneous anesthesia varied from 2.4 to 7.2 decibels. In order to obtain an index of the reliability of these small differences (in addition to the obvious regularity with which they occurred) a χ^2 was computed for each group of 100 measurements according to the method described by Fisher (**10**). These χ^2s varied from 47.2 to 140.9. χ^2 values of these magnitudes clearly show that cutaneous anesthesia is not unrelated to diminished vibratory sensitivity. Had the χ^2 values been as low as 5, the probability of such a diminution occurring on a chance basis would still have been only 1 in 100. Therefore, it may be concluded that cutaneous anesthesia causes a small but reliable diminution of vibratory sensitivity.

It is also evident from Table III that no appreciably greater effect was obtained after anesthetization of the 9 sq. cm. area. With three subjects the loss in sensitivity was a fraction of a decibel greater and with one subject a fraction of a decibel less when the larger area was anesthetized. These fractional differences are of no significance since they fall within the limit of error of the instrument. It is evident from these results that only those tactile end-organs in the immediate vicinity of the stimulus are excited with sufficient intensity to contribute to the vibratory experience. It is of

course possible that in regions where the physical vibrations are propagated with less damping or where the density of the touch receptors is greater, the effect of tactual anesthesia may be somewhat greater. However, under the present experimental conditions it is clear that the role of the tactile receptors is relatively small and the preponderance of the impulses which give rise to vibratory experience must be contributed by receptors in the subcutaneous tissues. This fact helps to clarify one of the striking qualitative characteristics of pallesthesia, namely the perception of the vibrations not only at the surface but also 'under the skin' in a manner akin to the arm 'going to sleep' or to the experience of '*ameisenlaufen*.'

Discussion

Two facts of general importance for the theory of pallesthesia have been demonstrated by the present experiment. First, a consistent decrease in vibratory sensitivity occurred with cutaneous anesthesia. This result was obtained in 23 of the 26 individual experiments in which tactual anesthesia was complete. The three exceptional cases have been shown to be due to a peculiarity in the localization of the sensation in certain regions of the arm. Second, the change in vibratory sensitivity resulting from cutaneous anesthesia was very small in comparison to the marked change in tactual sensitivity. Whereas touch was abolished in the anesthetized area, vibratory sensitivity was reduced by only a few decibels. In order to answer the possible objection that the smallness of the effect was due to the participation of tactile receptors beyond the anesthetized area, a larger area was anesthetized. The resulting increase in the number of narcotized touch receptors produced no additional loss of vibratory sensitivity.

These facts lead to the view that the peripheral mechanism of vibratory sensitivity is of a dual nature and involves both superficial and deep pressure receptors.[3] The consistency

[3] The results of the experiment do not offer direct evidence either for or against the existence of a separate 'vibratory sense' (20, 21). However, they make the hypothesis of such a sense more complicated by necessitating the further assumption of a system of 'deep vibratory receptors.' Even the simpler form of this theory, which has never been widely held, has been questioned both because of its dubious tenets and the disproof of certain lines of evidence cited in its favor (9, 11, 27).

with which this small decrease in vibratory sensitivity was obtained with cutaneous anesthesia suggests that touch receptors normally participate in the response to vibratory stimuli. Since the vibratory sensitivity of the affected region was still close to normal when tactual anesthesia was complete, it is obvious that mechanical receptors other than those of superficial touch must also be capable of responding to vibrations.

There is ample evidence in the clinical and experimental literature to support the view that both superficial and deep mechanical receptors may contribute impulses that give rise to vibratory sensation. In regard to the superficial receptors, several experiments which directly demonstrate an involvement of the tactual end-organs in vibratory sensitivity have been reported. von Frey (**12**) stimulated single sensory spots of the skin with weak faradic currents and with bristles attached to an electrically driven tuning fork. He claimed that only the pressure spots gave a characteristic intermittent sensation. Pain and temperature spots gave a continuous sensation with a prickling or thermal component. von Frey's general conclusion has recently been confirmed by Geldard (**15**) and Gilmer (**17**). These latter investigators report that with very light punctiform stimulation there is a marked difference in the vibratory sensitivity of 'pressure sensitive' and 'pressure insensitive' spots.

Evidence for a subcutaneous component of vibratory sensitivity is based largely on clinical studies which have revealed a striking dissociation of touch and vibration in pathological conditions (**6**, **7**, **8**, **28**, **29**). While the clinical tests of vibratory sensitivity have been very crude, the consistency with which this dissociation has been reported in cases of serious impairment of the central nervous system is significant. In tabes dorsalis, it has been found that the vibratory sensation is abolished in the region of serious proprioceptive disturbances whereas touch in this region remains intact. Similarly, in the Brown-Sequard syndrome, which involves a transverse hemi-section of the cord, vibratory loss and motor disturbances are found on the same side as the

lesion while tactual impairment occurs on the contralateral side. These facts indicate that there are central pathways for vibration which proceed uncrossed to the higher nerve centers as a part of the proprioceptive system and which are distinct from the principal cutaneous tracts. In these investigations minor disturbances of vibratory sensitivity associated with tactile disorders have apparently escaped detection.

The majority of previous investigators have claimed, with an apparent disregard of the work of others, an exclusively tactual or exclusively subcutaneous origin for the vibratory sensation. Either view appears erroneous upon consideration of all the evidence including the results of the present experiment. On the basis of the present knowledge concerning the peripheral mechanism of vibratory sensitivity, it may be stated that any receptor of the skin or underlying tissues which is capable of responding to mechanical stimulation may give rise to the vibratory sensation.

Summary

1. The purpose of the present experiment was to investigate the nature of the peripheral mechanism of vibratory sensitivity.

2. The procedure consisted in determining the absolute thresholds for touch and vibration before and after cutaneous anesthesis. Anesthesia was obtained by an electroendosmotic method which permitted an accurate control of the number of touch receptors anesthetized with little likelihood of affecting end-organs in the subcutaneous tissues.

3. There was a complete loss of tactual sensitivity with electroendosmotic anesthesia of a 4 sq. cm. area of the skin. Under the same conditions there was a very slight but a consistent decrease in vibratory sensitivity.

4. The absence of a greater loss in vibratory sensitivity might be due to a spread of stimulation along the surface of the skin or to the response of receptors in the underlying tissues. The fact that an increase in the size of the anesthetized area to 9 sq. cm. caused no additional loss in vibratory sensitivity disproves the former possibility.

5. The results of the experiment are most adequately explained on the theory that both superficial and deep mechanical receptors contribute impulses that give rise to vibratory experience. This view is also most consistent with the experimental and clinical data on the nature of the sensory mechanism of pallesthesia.

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QUANTITATIVE STUDIES OF THE INTERACTION OF SIMPLE HABITS. I. RECOVERY FROM SPECIFIC AND GENERALIZED EFFECTS OF EXTINCTION [1]

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INTRODUCTION

This study was designed to obtain a preliminary quantification of two behavior principles used by Hull (**6**) in his derivation of more complex behavior. They are: (1) the rate of recovery of habit strength as a function of time interval following extinction,[2] and (2) the rate of recovery of a habit as a function of time interval following the extinction of a second habit. These will be referred to respectively as recovery from (1) specific, and (2) generalized, effects of extinction. Two bar-pressing habits similar to those described by Skinner (**10**) and Youtz (**14**) were used in the experiment.

Youtz (**14**) found with a horizontal-bar-pressing habit that after extinction to a point where no responses were made for twenty minutes, a second extinction twenty-four hours later showed approximately 55 percent recovery. Miller and Stevenson (**7**) found negative acceleration in the rate of recovery of speed of running a straight alley. One part of the present experiment is designed to plot the amount of recovery

[1] The writer wishes to express his appreciation of the constructive criticism of this work by Professor Clark L. Hull. The experiment is a part of the coördinated research program of the Institute of Human Relations.

[2] It should be pointed out that the term 'extinction' originally referred to the weakening of a response tendency as a result of omitting the unconditioned stimulus, whose chief function was thought to be that of evoking the response. In the present experiment the reinforcement (strengthening) of the habit is caused by a food reward, which, since it follows the response, could not have evoked it. Thus the weakening of the response tendency by non-rewarded performance is not, conceptually at least, the same as extinction. However, in the absence of an adequate and concise substitute, the conventional term 'extinction' is used here.

of the bar-pressing response from *specific* extinction effects as a function of time following extinction. A second part is intended to plot the amount of recovery of the bar-pressing habit from *generalized* extinction effects, under conditions as similar as possible to the first. Youtz (**16**) has shown convincingly that the extinction of one bar-pressing habit reduces the strength of another similar habit. If extinction is a state of relaxation, as suggested by Wenger (**13**), then presumably the curves of recovery from the specific effects and from generalized effects of extinction should be parallel. Although this experiment does not offer a direct test of Wenger's hypothesis, the data obtained should have a bearing upon it.

Subjects and Apparatus

Two hundred albino rats from sixty to one hundred days of age were used as subjects. All except eight were male. The females were distributed among the eight experimental groups. Twenty-nine additional animals were discarded in the course of the experiment, fourteen for failure to learn, and fifteen because of extreme measures on one of the balancing criteria. Eight of the animals, distributed equally among the experimental groups, had been used previously in a maze experiment. The remainder had had no training of any kind.

The apparatus consisted of four separate double-walled boxes, similar to those used by Skinner (**10**) and Youtz (**14**), and a constant-speed polygraph having eight markers. The experimental boxes were insulated with three inches of rock wool. The rat's compartment of each box, twelve by twelve by twelve inches, was lighted by means of a 7.5-watt bulb resting on a double-paned observation window in the top of the box. The laboratory vacuum system supplied ventilation. One end of the rat's compartment was formed by a panel through which a vertical and a horizontal bar (see Fig. 1) could be introduced by means of levers outside the box. When either bar was retracted behind the panel, a shutter automatically closed the slot in the panel through which the bar had been extended.

When the bars were in the position where they were operated by the rat, they projected one-half inch from the panel. A downward pressure of fifteen grams and a sidewise pressure to the left of eight grams were required to move the horizontal (*H*) and the vertical (*V*) bars respectively. The movement of each bar was recorded by a separate marker on the polygraph. The mercury-cup switches operated by the bars were designed to record movements of three-sixteenths inch or more, the maximum movement being three-eighths inch. Slight jiggling movements not involving a definite pressure response did not activate the markers.

A knife switch on the outside of each box could be set so that a recorded pressure on either bar would operate an electromagnetic food mechanism which delivered a small pellet of food to the tray below the bar. The accompanying click of the mechanism was heard as a thud inside the box. The pellets of food were made from a calf-meal mash rolled into one-eighth-inch cylinders and cut into one-quarter-inch sections, each pellet weighing .05 gram when dry.

Procedure.—For two or more weeks the subjects were housed in the laboratory and fed at the usual time of beginning experimentation in order to establish a feeding and activity rhythm. Three days before being used in the experiment the animals were placed in individual cages and each day fed all they would eat in a one-hour period. During the experiment proper they received only the 120 pellets incidental to habituation and training.

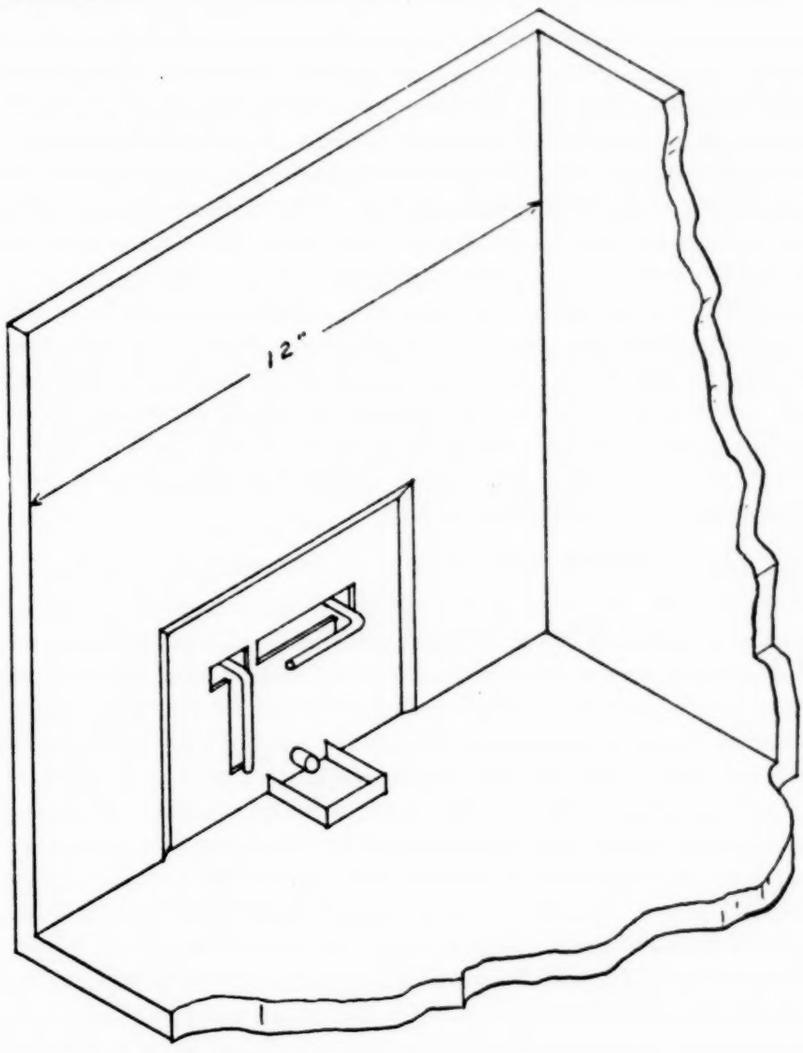

FIG. 1. View of panel end of rat's compartment in the experimental boxes. Both bars are shown in position to be operated. During the experiment, only one bar was presented at a time. When either bar was withdrawn, a metal shutter automatically closed the opening in the panel.

The two problems involved in this study, (1) the rate of spontaneous recovery from the primary effects of extinction and (2) the rate of recovery from the generalized effects of extinction, were approached by separate experiments, each having four sub-groups. The use of four experimental boxes made it possible to run four rats at a time, one in each sub-group. In alternate three-day periods (the time required to complete the procedure with four animals in one experiment) four rats were run in each experiment. Habituation and training procedures were the same in both experiments. In

Experiment I, on the third day the horizontal-bar habit was extinguished and its recovery measured after four different intervals, using a separate group of twenty-five animals for each interval. In Experiment II, the vertical-bar habit was first extinguished and the recovery of the horizontal-bar habit tested after the same four intervals. A detailed account of the daily procedure is given below.

Day 1. Habituation to Box and to the Click of the Food Mechanism

The four animals were placed in the experimental boxes late in the evening and left overnight. In the morning they were returned to their living cages for ten minutes, during which time ten pellets of food were placed in the food tray of each box. The animals were then returned to their respective boxes and allowed to eat the food and explore for thirty minutes. Two more similar half-hour habituation periods were given, each followed by the ten-minute intermission. After the intermission following the last period, the animals were again placed in the boxes, this time with no food in the tray. During this period thirty pellets were given one at a time by the experimenter, who operated the food mechanism by means of a switch outside the box. Each pellet was delivered only after the preceding one had been eaten. By this procedure the animals learned to respond at once to the sound of the mechanism by obtaining the food. This was done with the expectation that they would secure the reward quickly after they had pressed the bar during the training on the following day, and thus learning would be facilitated. After thirty pellets had been given in this way the animals were returned to their living cages.

Day 2. Training of the Vertical and Horizontal Bar Habits

The animals were placed in their respective boxes, the food mechanism circuit connected so as to deliver food when the bar was depressed, and the *H*-bar introduced. A piece of moist food was placed behind the panel so that the food odor was drawn through the slot behind the bar by the ventilating system In investigating this odor, the animals sooner or later incidentally pressed the bar and a pellet was automatically delivered to the tray. The bar was withdrawn after fifteen such reinforced trials, and the animals returned to their living cages for thirty minutes. Then fifteen more reinforced trials were given, these with the odor stimulus removed. A similar training of the *V*-bar habit was begun thirty minutes later, again with the odor stimulus for the first fifteen trials only. Thus thirty reinforcements were given on each habit.

Not all the animals learned immediately. In cases when ten or fifteen minutes elapsed without a response on the bar, the back of the experimental box was opened and one or both of two expedients employed. An electric fan was turned on to increase the current of odor-laden air passing through the slot behind the bar, or a screw-driver with a small amount of moist food smeared on it was pushed through the slot and rattled gently. If still no response occurred within ten minutes or so, the animal was discarded.

Day 3. Extinction and Tests of Recovery

Habituation and training days were the same for all animals. A different procedure was used for the animals in Experiment I and Experiment II on the third day.

Procedure of Experiment I. Recovery from the Specific Effects of Extinction.— The four animals were placed in their respective boxes and the *H*-bar introduced immediately, with the switches set to record responses but not to operate the food mechanism. Each animal was permitted to press the bar until a period of five minutes elapsed in which it had made no responses. At the end of this period the bar was retracted behind the panel and the rat returned to its living cage for the remainder of the re-

covery interval assigned to that particular animal. The recovery intervals used were 5.5, 25, 65, and 185 minutes, measured from the time of the last response preceding the 5-minute criterion period. At the end of the interval each rat was again placed in its experimental box and given a second period of extinction on the *H*-bar habit to the same 5-minute criterion, to test recovery.[3]

Procedure of Experiment II. Recovery from the Generalized Effects of Extinction.— The procedure was the same as that for Experiment I except that the first period of non-reward was given on the *V*-bar habit. Recovery was measured by test of the *H*-bar habit as before.

Twenty-five animals were used in each of the eight groups. The four sub-groups in Experiment I and the four in Experiment II were balanced by selection of group members on the basis of recorded performance on days 2 and 3 with respect to (1) learning time for both habits and (2) number of responses made during the first ex-

TABLE 1

EQUIVALENCE OF EXPERIMENTAL GROUPS

Measures of learning time and extinction. Experiments I and II can be compared only for learning time, since the first extinction was on the *H*-bar habit in Experiment I and on the *V*-bar habit in Experiment II.

A. Experiment I. First Extinction on *H*-bar Habit

Measure	Group (Recovery Interval Used)				Mean of all 100 Cases
	5.5 min.	25 min.	65 min.	185 min.	
H-bar learning time in minutes	10.9	11.0	10.9	10.3	10.8
V-bar learning time in minutes	13.2	11.5	8.9	10.4	11.0
No. responses, first extinction	49.9	50.0	48.9	50.1	49.7
Time for extinction in minutes	15.0	16.3	15.7	17.3	16.0
Rate (mean interval between responses in minutes)	.31	.39	.33	.37	.35

B. Experiment II. First Extinction on *V*-bar Habit

Measure	Group (Recovery Interval Used)				Mean of all 100 Cases
	5.5 min.	25 min.	65 min.	185 min.	
H-bar learning time in minutes	11.0	11.4	11.1	11.0	11.2
V-bar learning time in minutes	13.6	12.0	9.9	12.5	12.0
No. responses, first extinction	42.4	42.2	41.0	42.1	42.0
Time for extinction in minutes	12.7	11.0	10.7	12.8	11.9
Rate (mean interval between responses in minutes)	.32	.27	.22	.42	.31

[3] The animal in the 5.5-minute-interval group was carried to its living cage and placed inside; the cover of the cage was closed and opened, and the animal immediately returned to the box for the test of recovery. This procedure occupied 30 seconds which, added to the 5-minute criterion period, accounts for 5.5 minutes of recovery in this group. The additional recovery time with the remaining groups was spent in the living cages.

tinction. This selection resulted in a random distribution of the four groups in each experiment among the four experimental boxes. The large number of animals employed and the alternation in the running of Experiments I and II were relied upon to make the two experiments comparable. Table 1 gives measures by which the success of balancing may be judged. Because of the difficulty of equalizing extreme cases with only twenty-five animals in each group, rats giving more than 125 responses during the first non-reward period on either habit were excluded from the regular groups. The figure 125 was selected arbitrarily early in the experiment as being approximately twice the mean number of responses made during the first extinction. This caused the elimination of eleven animals in Experiment I, and four animals in Experiment II.

Results and Discussion

I. Relationship between Learning and Extinction

The correlation (r) between total time required to make thirty reinforced responses during the learning and the number of responses made before the 5-minute criterion was reached during the first extinction on the following day was $-.14 \pm .06$ for both the horizontal and vertical bar habits. These correlations are based on 111 and 104 cases respectively, the animals making more than 125 responses being included here

TABLE 2

Data for Experiment I. Measures of Recovery from the Specific Effects of Extinction

Means for recovery groups are based on 25 animals with the exception of those for (c) rate of response. Since this measure (mean interval between responses) was obtained by dividing the total time from first to last response by ($N - 1$) responses it does not include cases where less than two responses were made.

Measure of Recovery	First Period of Extinction on H-bar Habit (N = 100)	Recovery Interval Group			
		5.5 min.	25 min.	65 min.	185 min.
(a) No. responses					
Mean	49.7	7.6	14.4	19.5	24.4
σ_m	2.6	1.7	1.8	3.9	4.5
Median	43.3	3.0	13.5	18.0	21.0
(b) Time to reach criterion (min.)					
Mean	16.0	2.6	5.0	8.4	11.6
σ_m	.97	.64	.92	1.24	1.76
Median	15.5	1.2	4.1	7.4	9.0
(c) Rate (mean interval between responses) (min.)					
Mean	.35	.42	.36	.54	.66
σ_m	.02	.07	.05	.07	.14
Median	.33	.42	.34	.34	.50
No. cases	100	17	22	23	25

although excluded from the experiment proper. These figures differ radically from those found by Youtz (**14, 15**), who obtained a rho of $+.69 \pm .18$ (based on ten rats for the corresponding measures with twenty-four hours between learning and extinction, and $-.77 \pm .14$ (based on nine rats) when the extinction occurred immediately at the conclusion of learning. Youtz trained only the horizontal-bar habit,

TABLE 3

RELIABILITIES OF MEASURES OF RECOVERY FROM SPECIFIC EFFECTS OF EXTINCTION

Critical ratios (Diff./$\sigma_{diff.}$) of the means for the four recovery-interval groups. The first column shows the pairs of groups for which C.R.'s are given.

Paired Recovery-Interval Groups	Measures of Recovery		
	No. of Responses	Time Required to Reach Criterion	Rate of Response
5.5 min. vs. 25 min.	C.R. = 2.75	C.R. = 2.14	C.R. = 0.70
5.5 min. vs. 65 min.	= 2.80	= 4.16	= 1.21
5.5 min. vs. 185 min.	= 3.49	= 4.81	= 1.53
25 min. vs. 65 min.	= 1.19	= 2.20	= 2.09
25 min. vs. 185 min.	= 2.06	= 3.32	= 2.73
65 min. vs. 185 min.	= 0.82	= 1.49	= 0.76

giving forty reinforcements, ten per day for four days, and used a 20-minute criterion of extinction. The complete reversal in Youtz's results, together with his small number of cases, makes it seem likely that the small negative correlation found in the present experiment more nearly represents the true figure, at least when extinction occurs on the day following learning.

II. Recovery from the Specific Effects of Extinction

Youtz (**14**) has demonstrated spontaneous recovery of the bar-pressing habit following its weakening by non-reward (extinction). In a later paper (**16**) he also showed that the extinction of one bar-pressing habit weakened a second similar habit. The present experiment extends this work to plot the curve of recovery in both situations under comparable conditions. This section presents spontaneous recovery in terms of three measures: (1) number of responses made before the 5-minute criterion was reached, (2) time from first to last

response, and (3) rate of response, *i.e.* the mean interval between responses. This last measure is obtained by dividing time by the number of responses, separate scores being calculated for each rat. The means, σ_m's, and medians for these three measures are presented in Table 2 for each of the four recovery intervals. The reliability of difference between means for the four recovery interval groups is given in Table 3.

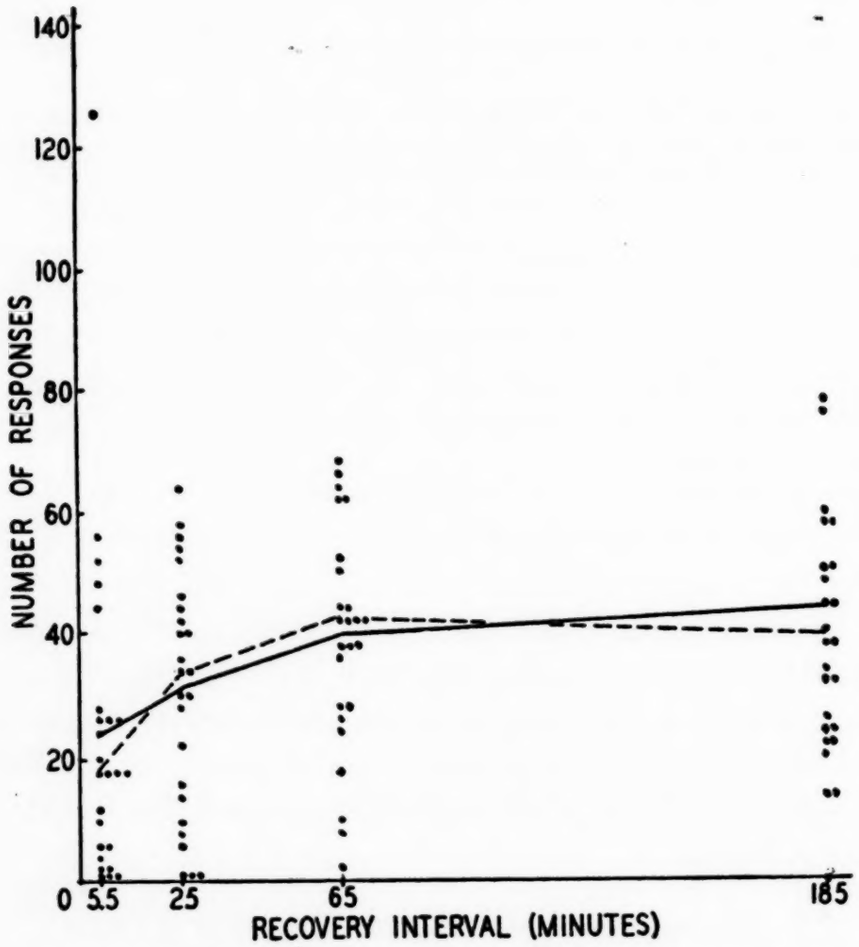

FIG. 2. Distribution of number of extinction responses made by the four groups in Experiment I during tests of recovery from the effects of extinction. Means are graphed by the solid line, and medians by the broken line.

Figure 2 shows the distributions of one of these measures (number of responses), together with the mean and median curves. These distributions are fairly typical of those for the other measures.

Figure 3 presents the recovery in terms of mean number of responses, together with the empirical equation describing

the function. It can be seen from Fig. 2 that the distribution for the 5.5-minute-interval group is decidedly skewed, causing the mean to fall considerably above the median in this group. For this reason the equations for the medians

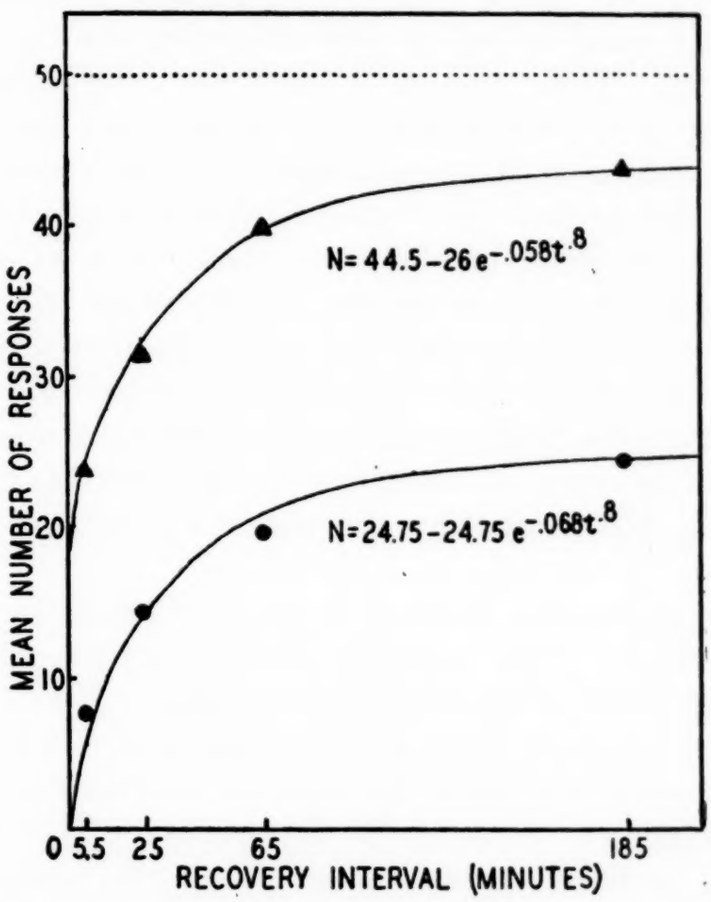

FIG. 3. Recovery as measured by mean number of responses. Recovery from the specific effects of extinction is shown by the small circles and recovery from generalized effects, by the small triangles. The smooth curves in both cases are obtained by plotting the empirical equations which have been fitted to the data shown. The horizontal dotted line shows the mean number of responses made during the first extinction of the *H*-bar habit by the Experiment I animals. Equations fitted to the median numbers of responses, corresponding to the means shown here, are as follows (N = median No. responses and t = recovery interval in minutes):

Experiment I. $N = 21.5 - 21.5\, e^{-.075t^{.8}}$
Experiment II. $N = 42.0 - 32.0\, e^{-.093t^{.8}}$

were also calculated, and are presented with Fig. 3. However, the mathematical expressions show only slight differences.

The generality of these mathematical statements of recovery can be determined only by further experimentation

of two kinds. Recovery as such may be plotted under different conditions, or deductions based on these equations in combination with other principles may be tested experimentally. The former type of experiment is represented by the work of Miller and Stevenson (7), who measured recovery of running speed following nine extinction runs in a straight alley maze. Their curves of recovery differ from that of Fig. 3 in that almost 100 percent recovery occurs in 55 minutes, and initial recovery is relatively much faster, causing a sharp break in the curve at the shortest interval. Miller and Stevenson attribute the break in their curve to an artifact of their experimental procedure, so the shape of their curve cannot be compared with the curve presented in Fig. 3. However, the percentage of recovery is clearly different in the two experiments, which indicates that the equations obtained in the present experiment can only be considered as representative of the rate of recovery and not as a general statement of a law.

Figure 4 presents recovery in terms of mean time required for extinction. This measure shows negative acceleration similar to that of number of responses. However, using time as a measure, there was 72.5 percent recovery in 185 minutes, while measured by number of responses there is only 49.1 percent recovery in that period. This difference, it should be noted, was obtained when two measures were taken on the same group of animals at the same time. Both might be used as measures of 'habit strength,' or 'excitatory tendency.' One conclusion which may be reached is that such terms as 'habit strength,' 'excitatory tendency,' etc., should always be qualified by the phrase, 'as measured by. . . .'

A third measure of recovery (rate of response) is shown graphically in Fig. 5. This measure, which would seem to correspond most closely to that of Miller and Stevenson (7) (running speed), differs from it even more than the measures discussed above. None of the differences between groups was acceptably reliable for this measure (see Table 3). However, it should be noted that the difference between the

25-minute and the 185-minute groups, which has a probability of 99.7 in 100 of being real, is in a direction counter to expectation. According to number or responses and time measures, the habit was stronger in the 185-minute group, yet the animals responded at a much slower rate. Since the

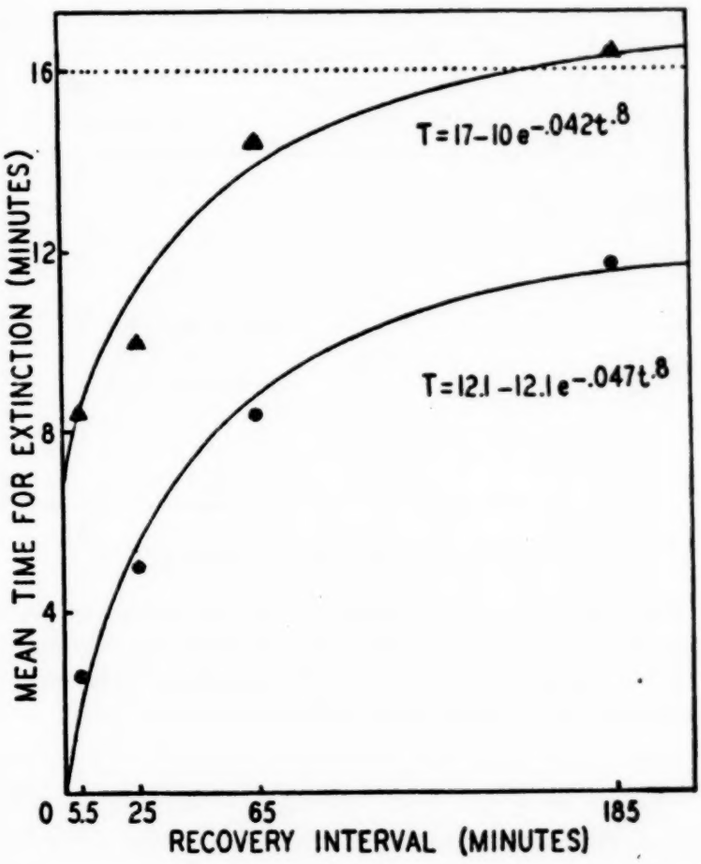

FIG. 4. Recovery from specific and generalized effects of extinction as measured by the time required to reach the 5-minute criterion in the tests of recovery. Means for recovery from specific effects of extinction are given by the small circles and those for recovery from generalized effects by the small triangles. The smooth curves in both cases are obtained by plotting the empirical equations which have been fitted to the data shown. The horizontal dotted line shows the time required for the first extinction of the *H*-bar habit by Experiment I animals.

reliability of the rate measure was comparatively low, no theoretical explanation is attempted here. If a similar relationship were found in other experiments, however, the phenomenon would demand more study, since it is the reverse of the somewhat analogous relationships of frequency and latency with other measures of conditioned-response strength.

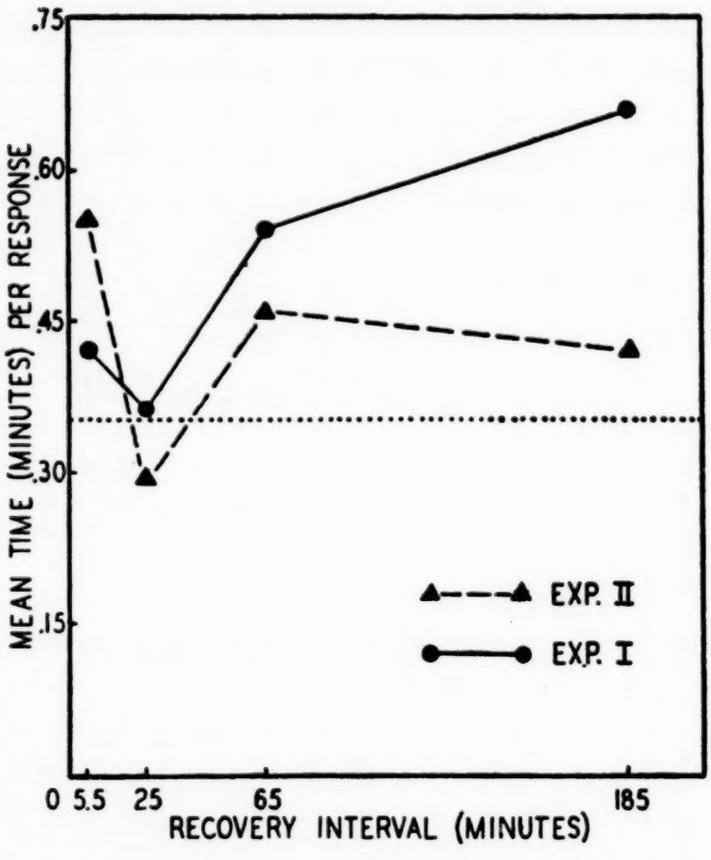

FIG. 5. Mean interval between responses during tests of recovery. Means for Experiment I are given by the small circles, and those for Experiment II by the small triangles. The horizontal dotted line shows the mean interval between responses during the first extinction of the *H*-bar habit in Experiment I.

III. Recovery from the Generalized Effects of Extinction

As was mentioned above, Youtz (**16**) showed that the extinction of one bar habit reduces the number of responses made during the immediately following extinction of a second habit. This section of the present paper demonstrates the recovery from these 'generalized effects of extinction.' Again three measures of recovery are used. These measures were obtained in the extinction of the horizontal-bar habit beginning 5.5, 25, 65, and 185 minutes after the last response of the vertical-bar-habit extinction. Means, σ_m's, and medians for number of responses, time, and rate of response are given in Table 4. The distributions of number of responses are plotted for the four groups in Fig. 6. In Figs. 3, 4, and 5 the mean values for the three measures are plotted together with the empirical equations for recovery in terms of responses

TABLE 4

DATA FOR EXPERIMENT II. MEASURES OF RECOVERY FROM THE GENERALIZED EFFECTS OF EXTINCTION

The figures in the second column are taken from the first extinction of the *H*-bar habit in Experiment I. Means for recovery groups are based on 25 animals with the exception of those for (*c*) rate of response. Since this measure (mean interval between responses) was obtained by dividing the total time from first to last response by $(N - 1)$ responses it does not include cases where less than two responses were made.

Measure of Recovery	First Period of Extinction on *H*-bar Habit ($N = 100$)	Recovery-Interval Group			
		5.5 min.	25 min.	65 min.	185 min
(*a*) No. responses					
Mean	49.7	23.9	31.2	39.6	43.9
σ_m	2.6	5.5	3.9	3.6	5.2
Median	43.3	18.5	34.0	42.5	39.5
(*b*) Time to reach criterion (min.)					
Mean	16.0	8.5	10.0	14.6	16.4
σ_m	.97	1.85	0.87	1.45	1.92
Median	15.5	6.5	9.5	13.5	17.2
(*c*) Rate (mean interval between responses) (min.)					
Mean	.35	.55	.29	.46	.42
σ_m	.02	.10	.03	.08	.05
Median	.33	.33	.28	.36	.38
No. cases	100	21	22	25	25

and time. Recovery measured by number of responses and time appears again to be fairly regular and negatively accelerated, while rate measures show reversals.

The critical ratios of the differences between the means (Table 5) are with few exceptions smaller than the corresponding ones for Experiment I. Given in order from greatest to least reliability, the measures are time, number of responses, and rate of response. This was true also for recovery from the specific effects of non-reward (Table 3), which disagrees with Youtz's finding (**14, 15**) that the number of responses was a more reliable measure than the time required to make these responses.

The differences between corresponding measures of mean recovery from specific and generalized effects of extinction are approximately equal for number of responses and time measures. In other words, the effect of lapse of time following the extinction has approximately the same effect upon

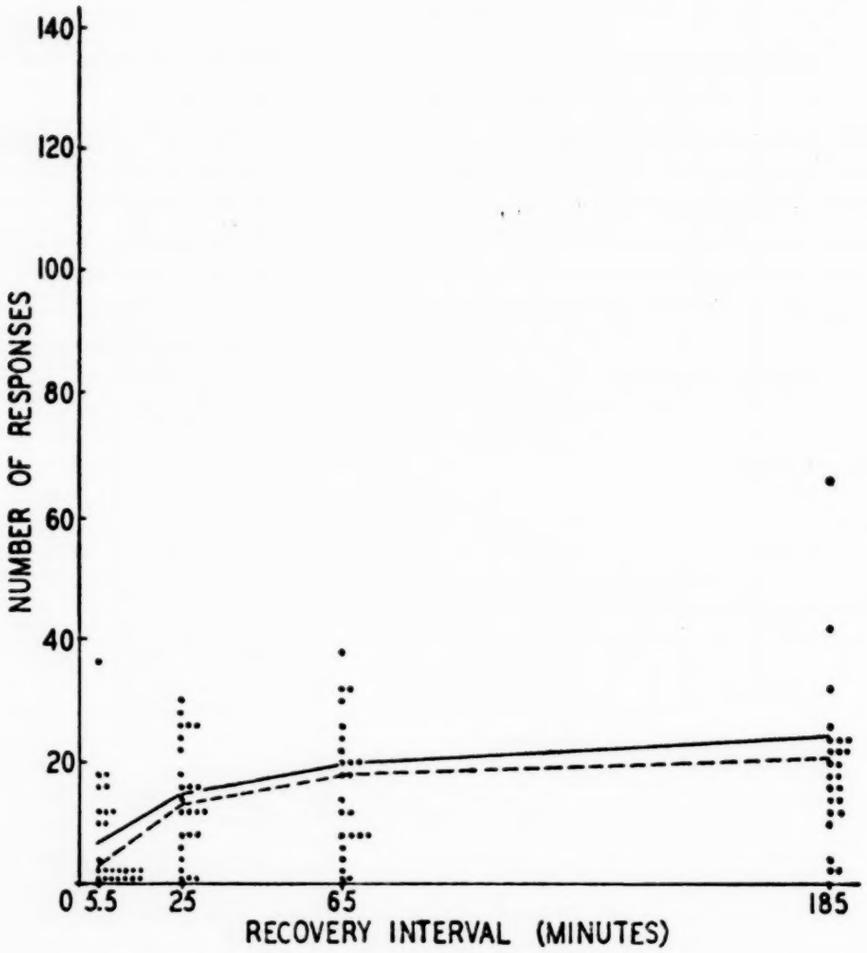

FIG. 6. Distributions of number of responses made by the animals in the four groups of Experiment II, recovering from the generalized effects of extinction. Means are shown by the solid line, and medians by the broken line.

TABLE 5

RELIABILITIES OF MEASURES OF RECOVERY FROM GENERALIZED EFFECTS OF EXTINCTION

Critical ratios (Diff./$\sigma_{diff.}$) of the means for the four recovery interval groups. The first column shows the pairs of groups for which C.R.'s are given.

Paired Recovery-Interval Groups	Measures of Recovery		
	No. of Responses	Time Required to Reach Criterion	Rate of Response
5.5 min. vs. 25 min.	C.R. = 1.07	C.R. = 0.73	C.R. = 2.50
5.5 min. vs. 65 min.	= 2.39	= 2.59	= 0.70
5.5 min. vs. 185 min.	= 2.64	= 2.96	= 1.16
25 min. vs. 65 min.	= 1.58	= 2.72	= 2.00
25 min. vs. 185 min.	= 1.95	= 3.04	= 2.24
65 min. vs. 185 min.	= 0.68	= 0.78	= 0.43

the absolute amount of recovery from specific and generalized extinction. This point can be better appreciated by subtracting mean or median values in Table 2 from the corresponding values in Table 4. The results of these subtractions, representing differences in the absolute measures of recovery from specific and generalized effects of extinction, are almost constant for the four recovery intervals.[4] This is supporting evidence for the dissipation of a general state of the organism which had resulted from the initial extinction. Wenger (**13**) suggests that this state may be identified as generalized relaxation. However, the evidence from the present experiment can be merely suggestive for two reasons: (1) only one set of conditions was tested and therefore the parallel effect may be just a coincidence; and (2) there is no assurance that the units of recovery are constant for different absolute values.

IV. Rate of Response during the Course of Extinction

The data were further analyzed to find the rate of response (mean interval between responses) for successive fifths of the total number of responses made by each animal during extinction. All groups were averaged for the first horizontal-bar-habit extinction and figured separately for the second (recovery test) extinction of each group. These data are presented in Table 6. It was noted that all four of the Experiment I groups showed a 'warming up' phenomenon, that is, an increase in rate of response during the second fifth of the total responses, while this phenomenon was seen in only one of the Experiment II groups. However, further analysis seems to indicate that this difference did not represent a qualitative dissimilarity in the effects of specific and generalized extinction. The presence or absence of the initial increase in rate of response is apparently determined by the strength of the response tendency being extinguished. When the animals are separated into two groups (those making 20 or less responses during extinction and those making 21 or more), the 'warming up' phenomenon is found only for those

[4] The apparent convergence of the two curves in Figs. 3 and 4 is thus shown to be almost entirely an optical illusion.

TABLE 6

MEAN INTERVAL BETWEEN RESPONSES FOR SUCCESSIVE FIFTHS OF THE TOTAL RESPONSES

The number of cases is usually less than 25 since only those animals making more than five responses are included.

Conditions of Extinction	No. Cases	Fifths of Total Responses				
		1	2	3	4	5
Exp. I. First extinction of *H*-bar habit	99	.15	.20	.33	.39	.63
Exp. I. Recovery test, second extinction of *H*-bar habit						
5.5-min. interval	10	.31	.24	.44	.69	.21
25-min. interval	20	.18	.14	.33	.46	.67
65-min. interval	22	.31	.30	.39	.67	.59
185-min. interval	22	.50	.38	.39	.52	.56
Exp. II. Recovery test on *H*-bar habit following extinction on *V*-bar habit						
5.5-min. interval	18	.24	.30	.40	.57	.56
25-min. interval	22	.11	.18	.22	.41	.52
65-min. interval	24	.24	.18	.36	.59	.55
185-min. interval	25	.15	.30	.34	.64	.72

making fewer responses (see Table 7). This was true for the first *H*-bar extinction and for the recovery test extinction of the Experiment II animals.

It might be supposed that dividing the total responses into

TABLE 7

INTERVAL BETWEEN RESPONSES FOR SUCCESSIVE FIFTHS OF THE TOTAL NUMBER OF RESPONSES MADE DURING EXTINCTION

The data are given separately for those animals giving less than 20 and more than 21 responses. Only those animals giving more than 5 responses are included.

Total Responses	No. Cases	Fifths of Total No. Responses				
		1	2	3	4	5
Exp. I. First extinction of *H*-bar habit						
20 or less responses	13	.22	.23	.21	.36	.49
21 or more responses	86	.14	.20	.34	.39	.65
Exp. II. Test of recovery from generalized extinction effects						
20 or less responses	20	.29	.27	.33	.51	.63
21 or more responses	69	.15	.22	.31	.43	.58

fifths obscured an initial increase in rate for the group making 21 or more responses because of the larger average number of responses included in each fifth. That this was not the case may be seen by reference to Table 8. The records for the Experiment I animals which made 21 or more responses during the first extinction of the *H*-bar habit were re-measured. The mean intervals between responses for the first five successive groups of three responses did not show any indication of a warm-up. The evidence as a whole seems to show that an increase in rate of response early in the course of extinc-

TABLE 8

MEAN INTERVAL BETWEEN RESPONSES FOR THE FIRST FIVE SUCCESSIVE GROUPS OF THREE RESPONSES DURING THE FIRST EXTINCTION OF THE *H*-BAR HABIT IN EXPERMENT I

Total Responses	No. Cases	Successive Groups of 3 Responses				
		1	2	3	4	5
21 or more.....	86	.11	.14	.20	.19	.19

tion is found only when the response tendency is relatively weak.

Switzer (**11**), Scott (**9**), Hudgins (**5**), and Hilgard and Marquis (**3**) found an initial rise in extinction curves. Hovland (**4**), investigating the conditions under which this rise occurred, found that it was favored by a large number of reinforcements, massed practice, and extinction immediately following training. In contrast, the phenomenon was found in this experiment where the habit was relatively weak and when non-reward occurred twenty-four hours after training. This would indicate that the inhibition-of-reinforcement hypothesis, by which Hovland explains his data, is not the factor involved here.

An hypothesis which accounts for the 'warming up' phenomenon very nicely is that suggested by Guthrie's approach (**2**). According to his analysis the conditioned stimuli for any response would include internal stimuli resulting from posture. Since during learning one response follows another,

the postures involved in making one response furnish stimuli to which the response itself is conditioned—circular conditioning. During extinction or non-reward, stimuli from the first few responses facilitate the occurrence of those following so that they should occur with greater frequency (until the weakening effects of non-reward overcome this trend).

If the previous conditioning had been very strong the external stimuli alone would cause a maximum rate of response at the beginning of extinction, so that no further increase in rate could occur. This last assumption would require that the initial rate for those animals making a large number of responses should be faster than for those giving few responses. Reference to Table 5 shows this to be the case.

In addition to Guthrie's concept of specific (circular) facilitation there is the possibility of a general facilitatory effect. Emotion or excitement is commonly associated with non-reward (frustration), and this is assumed to have an energizing effect upon any response which may occur. If this hypothesis were applied to the data, the difference in the measurable effect of this factor upon a strong or a weak tendency would, presumably, again be accounted for in terms of the initial maximal or submaximal rate.

Summary and Conclusions

Two hundred albino rats were given thirty food-rewarded training trials on a horizontal-bar-pressing habit and thirty on a vertical-bar habit. Half of these animals were later allowed to perform the *H*-bar habit unrewarded until they reached a criterion of no responses in a period of five minutes. Recovery of the *H*-bar habit was measured by a second extinction test performed 5.5, 25, 65, or 185 minutes after the last response, a separate group of twenty-five animals being used for each interval. With the remaining 100 animals, following extinction of the *V*-bar habit, the recovery of the *H*-bar habit was measured, using the same recovery intervals (recovery from the generalized effects of extinction). The criterion of no responses in five minutes was used for original extinction period and for all tests of recovery.

Measured by number of responses and time to reach the 5-minute criterion, recovery from the specific effects of extinction was negatively accelerated, showing approximately 50 percent and 75 percent recovery respectively in 185 minutes. The curves of recovery from generalized effects were approximately parallel to the curves of recovery from specific effects of extinction, but showed 85 percent and 100+ percent recovery in terms of number of responses and time measures respectively. Measured by mean rate of response, there was no recovery in the ordinary meaning of the term. The curves of recovery from both specific and generalized effects of extinction found by this measure showed marked reversals.

A finding which is implicit in the above results is the operation of generalization of extinction to weaken a habit which has not itself been extinguished. A somewhat analogous phenomenon has been demonstrated (**8**, p. 152, and **1**) with conditioned reflexes, but its operation in other forms of learning has been questioned (**12**, p. 110). The present experiment confirms the results of Youtz (**16**) in showing that the non-rewarded performance of one motor habit has a weakening effect upon a second habit for some time thereafter.

It should be remembered that Pavlov's 'irradiation of inhibition' involves but one response conditioned to several stimuli: extinction of the response to one stimulus reduces the amount of secretion when another conditioned stimulus is presented soon after (**8**, p. 163). The present experiment and that of Youtz (**16**) employ two responses, so that the two situations are clearly not identical. However, Thorndike has assumed them to be essentially similar (**12**, pp. 110–111), and it is the writer's opinion that the two phenomena are probably closely related. The frequency with which functional similarities are found between conditioned reflex and other phenomena supports the belief that there are certain basic 'principles of behavior' which underlie both.

Extinction curves were plotted as the rate of response in successive fifths of the total responses made during the extinction. Pooled curves for animals making twenty or less

extinction responses gave evidence of a 'warming up' phenomenon—an initial increase in rate of response. This was not found in the pooled results for animals making more than twenty responses.

(Manuscript received May 2, 1938)

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SPACED PRACTICE AS A TEST OF SNODDY'S TWO PROCESSES IN MENTAL GROWTH

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In a report of an extensive series of experiments on the learning of mirror-vision coördinations, Snoddy has presented evidence for two opposed processes in mental growth (4). Since he believes his results to be of revolutionary significance for psychology, they deserve careful consideration and experimental study in other situations. Because our earlier investigation of performances on the Koerth pursuit rotor (1) yielded results paralleling in certain respects those obtained by Snoddy, it appeared that the pursuit task might be feasible for use in testing his hypotheses. As a point of departure we accepted provisionally Snoddy's characterizations of primary and secondary growth, then designed an experiment which appeared to us to test these characterizations:

> Primary growth comes early and is enhanced by interpolated time; it approaches its maximum as the length of the interpolated time is increased. Secondary growth comes later and is enhanced by reducing the interpolated time; it is maximum when the interpolated time is zero, or when the practice may be said to be continuous (4, p. 15).

Since secondary growth rests upon primary growth, the most advantageous distribution of practice should be such as to encourage primary growth followed by secondary growth. The arrangement is obviously one in which spaced practice is utilized in the early portions of practice and massed practice in the later portions. In view of the characterizations of primary and secondary growth, the following theorem appears to be justified:

> *Theorem.*—If two equated groups of subjects are given the same number of practice trials distributed over the same total time, but the practice is differently distributed, that group which is given initial spaced practice and final massed practice should show

[1] The authors are jointly responsible for the plan of the experiment. One of us (L.R.D.) acted as experimenter throughout; the other (E.R H.) is responsible for presenting the results in their present form.

higher scores at the end of the period than the group which is given initial massed practice and later spaced practice.

The experiment to be reported is a test of this theorem based on Snoddy's statements concerning primary and secondary growth.

Experimental Procedure

The mechanical arrangements were the same as in our previous study. The subject attempts to hold the tip of a hinged stylus on a target mounted on the disc of a phonograph revolving at the rate of one revolution per second. A score of 10 per revolution, or 600 per minute, is possible. Any failure to keep the stylus on the target results in a smaller score. The score is obtained electro-magnetically. Results are reported from two groups of subjects tested individually, 25 in each group, all undergraduate students at Stanford University. The whole experiment with one subject, from the beginning of the first trial through the last, took exactly 43 minutes. There were 14 one-minute trials within this 43-minute period, distributed differently for the two groups. The groups are designated *E* and *F* to distinguish them from groups *A* to *D* of the earlier study.

The first 3 trials were alike for both groups, 1-minute trials with 1 minute rests, in order to serve as a basis for determining how well the groups were equated After these trials, group *E* was treated under circumstances favorable for learning according to the theorem by having greater elapsed time between the next few trials and massed practice later. Group *F* was treated unfavorably by having massed practice early and spaced practice later. The exact arrangement can be read from Table 1.

Results

The data are presented in Table 1. The scores of the first three trials indicate that the two groups were satisfactorily matched, the differences between average scores being insignificant. The final scores test the theorem which predicted that the scores of Group *E* would exceed those of Group *F*. On the contrary, *the final scores of Group F exceed those of Group E by a statistically significant amount.* The critical ratio of the difference in the last trial is 5.2.

Trial-by-trial averages are presented in Fig. 1. The usual advantages of spaced over massed practice in motor tasks are shown. Whether the spacing comes early or late the group which has the advantage of spacing temporarily spurts ahead. For example, after the third trial, in which both groups had scored approximately alike, Group *F* rested one minute, then scored 197 in the next trial. Group *E*, resting 11 minutes, scored 256 in the next trial. Thus this single

TABLE 1

SUMMARY OF SCORES ON THE PURSUIT ROTOR FOR THE TWO GROUPS DIFFERING IN THE DISTRIBUTION OF PRACTICE AND REST

Trials each 1 minute; maximum possible score per trial, 600. Standard errors are given where comparisons are to be made. Blanks indicate rest periods. Numbers in parentheses indicate trials.

	Minutes Elapsed since Beginning of Practice	Mean Scores per Trial	
		Group E (Decreasing Rests) $N = 25$	Group F (Increasing Rests) $N = 25$
Initial trials; conditions alike for all subjects	1	(1) 72 ± 12	(1) 72 ± 9
	2		
	3	(2) 119 ± 17	(2) 114 ± 13
	4		
	5	(3) 152 ± 18	(3) 160 ± 17
Distribution of practice and rest differs for the two groups	6		
	7		(4) 197 ± 16
	8		
	9		(5) 238
	10		
	11		(6) 278
	12		
	13		(7) 301
	14		
	15		(8) 315
	16		
	17	(4) 256 ± 19	(9) 329 ± 14
	18		
	19		
	20		
	21	(5) 291 ± 19	(10) 403 ± 13
	22		
	23		
	24		
	25	(6) 326 ± 18	(11) 440 ± 10
	26		
	27		
	28		
	29	(7) 368 ± 17	(12) 460 ± 9
	30		
	31	(8) 370	
	32		
	33	(9) 363	
	34		
	35	(10) 367	
	36		
	37	(11) 361	
	38		
	39	(12) 367	
	40		
	41	(13) 371 ± 16	(13) 474 ± 6
Final trial after 1-minute rest for subjects of both groups	42		
	43	(14) 374 ± 17	(14) 470 ± 7

longer rest sufficed to produce an appreciable difference between the scores of the two groups. Following the ninth trial the situation was reversed. Within this trial, Group *E* scored 363, and after a rest of one minute scored 367 on the tenth trial. Group *F* lagged behind Group *E* on the ninth trial, scoring 329, but a rest of three minutes permitted the score to rise to 440 on the tenth trial. It is to be noted that the introduction of spacing not only permits Group *F* to make

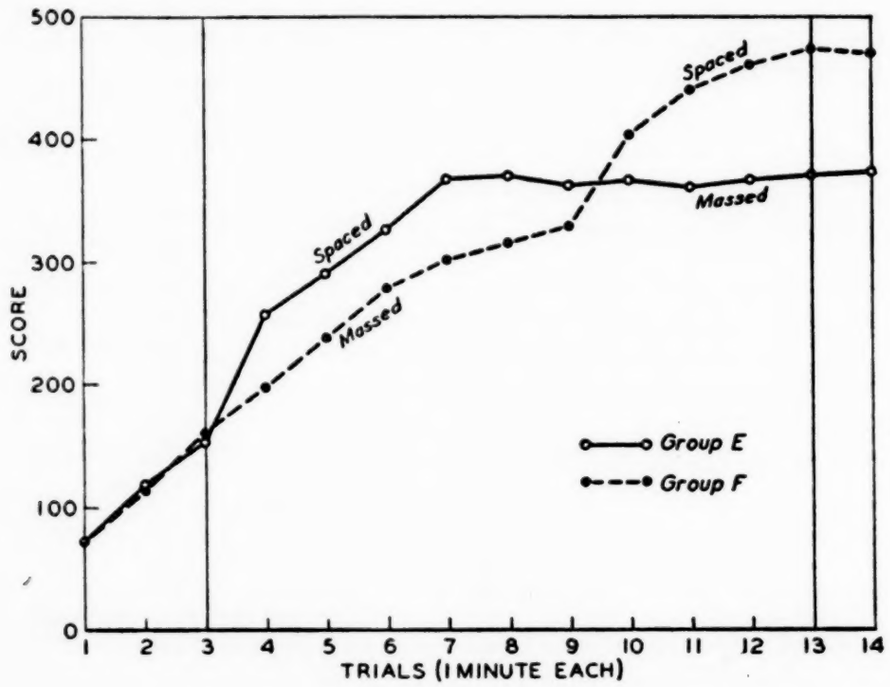

FIG. 1. Scores on the pursuit rotor with different distributions of practice and rest following trial 3. Abscissa in trial units. For Group *E* the rest intervals decrease progressively; for Group *F* they increase.

up for its losses due to massing, but to rise above Group *E*, and to end with higher scores. If the effects of spacing and massing were equal throughout the experimental period, the two groups would be expected to produce scores rising unequally, but ending at the same place. Since both groups have had the same number of trials, and Group *F* ends higher than Group *E*, the only conclusion that can be drawn is that a condition of increasing rests is more efficient than a condition of decreasing rests. This contradicts the theorem based on Snoddy.

Because of the importance which attaches to time, the data are presented in the form of scores minute-by-minute [2] in Fig. 2. When analyzed in this way it becomes clear that the scores of Group *F*, with increasing intervals between trials, are consistently above those of Group *E* after the initial period in which the groups were treated alike. Something is achieved within the early massed practice of Group *F* which is not lost when greater time intervals are added later in the

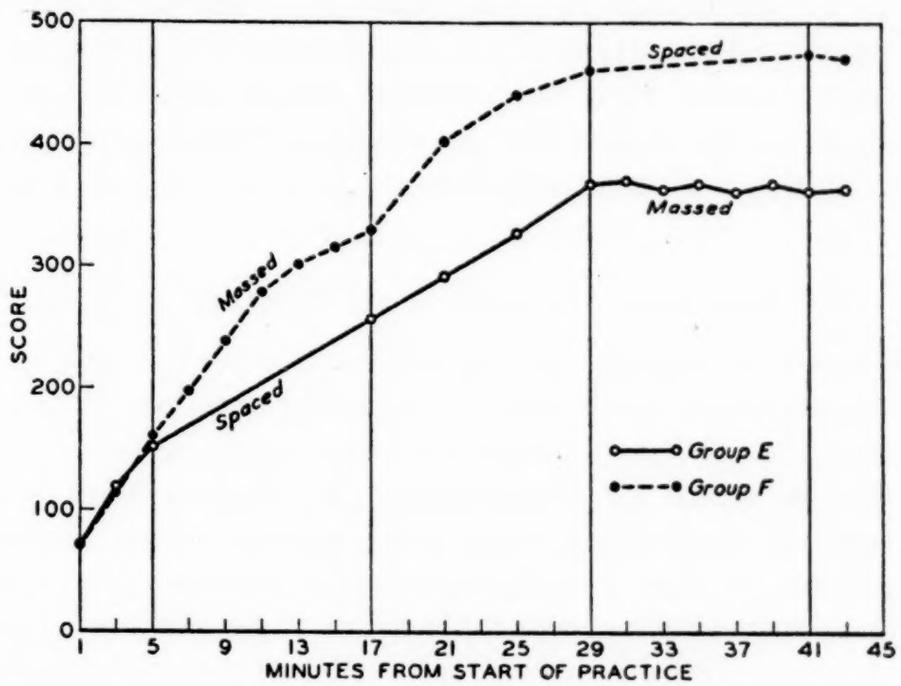

FIG. 2. Scores on the pursuit rotor with different distributions of practice and rest following the 5th minute. Abscissa in time units. For Group *E* the rest intervals decrease progressively; for Group *F* they increase.

hour. The conditions under which scores were made at the 21st, 25th, and 29th minute were strictly comparable for the two groups, and Group *F* maintained its advantage. The disadvantage of massed practice toward the end of the period for the subjects of Group *E* is manifested by the failure of the scores to rise. Although the scores of Group *F* are not rising

[2] Plotting the results of distributed practice experiments by elapsed time rather than by trials, an analytical procedure which we found useful in our earlier study, was suggested to us conversationally by Professor R. H. Wheeler. Acknowledgment to him was inadvertently omitted in the previous report. Since this procedure is likely to prove useful in other connections, we wish here to record its source, and to express our gratitude to Professor Wheeler.

much, they are closer to the maximum, and at this stage a small rise is more significant. The drop in the scores of Group *F* in the last minute, when a rest of only 1 minute has intervened, illustrates again the disadvantage of massing near the end of practice. Since the time-span is alike for both groups, the differences cannot be attributed to 'maturation' or 'growth' correlated with the passing of time. Since the trials were equal in number for the two groups, the results are not to be attributed to gross amount of stimulation. The factors related to early and late distribution of practice are the important ones. It is such factors which Snoddy has emphasized, but the results are just opposite from those predicted from Snoddy's characterizations of primary and secondary growth.

Is the Theorem Applicable to Pursuit Learning?

If scores in pursuit learning are too different from those of the mirror-vision coördination studied by Snoddy, it may be objected that the proposition we have tested is unfair to his theory. A study by Humphreys (**2**), dealing with scores obtained on the pursuit rotor, was prepared under Snoddy's supervision, so that Snoddy apparently accepted the scoring device as sufficiently comparable to his to be used as a test of this theory. There is supporting evidence that the functions are similar. The form of the learning curve under uniform practice conditions is essentially alike for the Snoddy task and the pursuit task as we have scored it. There are, of course, important qualitative differences between the mirror-vision task and the pursuit task, but this is not the point at issue at present. It may be concluded that it is not unfair to analyze pursuit scores as a test of the Snoddy theory.

A recent study by Renshaw and Schwarzbek (**3**) presents data on the form of the pursuit-meter function when inter-practice rests increase and decrease. The authors conclude:

> In spite of the fact that final performances do not differ greatly, it is clear that a progressively decreasing rest interim favors more rapid, and in all probability, more effective improvement than does a progressively increasing rest (**3**, p. 14).

Their stated conclusion therefore agrees with the theorem based on Snoddy, and contradicts our results. If this were a

genuine contradiction, it might be supposed that our disagreement with Snoddy was a function of some peculiarity within the pursuit task we chose. It becomes necessary to examine the evidence upon which Renshaw and Schwarzbek base their conclusion. The conclusion rests primarily on the results of their Group *D*, with decreasing rests, and their Group *I*, with increasing rests. Group *D* succeeds in making early good scores in fewer trials than Group *I*. This is comparable in our results to the initial success of Group *E*, as shown in Fig. 1. Renshaw and Schwarzbek do not present an analysis comparable to our Fig. 2. When their data are viewed in terms of elapsed time, it is found that *throughout the whole last half* of their experimental period, their Group *I* is averaging greater success than their Group *D*, and Group *I* ends with scores distinctly superior. Therefore, all the final advantage rests with Group *I*, with increasing rest intervals, rather than with Group *D*, with decreasing rest intervals.[3] In spite of the authors' conclusions, which give the opposite impression, their data are in substantial agreement with ours, and, like ours, they contradict the Snoddy principle that continuous trials should be advantageous late in practice. Therefore our results are not peculiar to the particular task we have chosen.

A Re-examination of the Evidence for Two Opposed Processes

Wherever factual comparisons are possible, the data from our experiments are not out of harmony with Snoddy's data. Thus when trials are separated at 1-minute intervals throughout practice, the scores obtained in our Group *C* (1, Table 1) may be fitted by a logarithmic curve as satisfactorily as scores from the comparable group of Snoddy's study (4, p. 78). We have shown distributed practice to lead to higher scores

[3] Because their trials vary somewhat in time required, it is not possible to make a precise chart of the course of improvement with elapsed time from the published data. A close approximation is possible on the assumption of trials of uniform length. Even in the trial-by-trial averages which Renshaw and Schwarzbek present (3, p. 10), Group I is shown to perform better than Group *D* on 9 of the last 10 trials. It does not appear from this that Group *D* is showing 'more effective improvement.'

throughout learning, both in the results reported here and in the earlier study (1, pp. 251–2); similar results are found in Snoddy's experiments (4, Table III). The disadvantages of continuous trials are serious enough to produce reductions in scores instead of increases, late in learning as well as early in learning. This was shown clearly in our Group *D* (1, Fig. 3), and the results are duplicated in Snoddy's study of interference (4, Fig. 5). A similar loss with excessive massing has been reported by Travis (5, Fig. 2). The overnight losses in the pursuit task have been shown by Humphreys (2, Fig. 2) to be similar to those in mirror-coördination (4, Fig. 6).

Our failure to confirm Snoddy's theory, while the experimental findings agree with his, suggests that his theory may not be supported adequately by his own data. There are many reasonable suggestions included in Snoddy's monograph. Learning should be related to growth. There may well be more than one aspect to a skilled visual-manual task such as mirror-coördination or pursuit learning. Interference with some aspects of the performances may interfere less with other aspects; rest pauses may have a differential effect on the several portions of the task. Snoddy's calling attention to such relationships within learning is commendable. We are not objecting to these reasonable suggestions; rather, we question whether Snoddy's logical, mathematical analysis has discovered and defined the two (and only two) processes in mental growth. Our doubts are based on several unsatisfactory aspects of his analysis, discussion of which follows.

(1) *The Cumulative Mean as a Measure of Primary Growth.* —In spite of the advantages of distributed practice, 24 hours between groups of trials may result in overnight losses. This is best shown by a group practicing 20 trials per day at half-minute intervals, resting 24 hours between sessions (4, Table VIII, Fig. 6). The overnight loss is accounted for as a subtraction of secondary growth, which is said to be temporary so that its rise is limited to the periods of practice.

In spite of the increasing overnight loss, each day begins at a point higher than that at which the preceding day began.

There is thus a stable learning underlying the unstable learning which is lost overnight. The stable learning is primary growth. A close approximation to the curve produced by joining the initial scores of each day can be found by taking the cumulative mean score of all the trials up to that point. Hence the cumulative mean is taken as the measure of primary growth, and all scores above the cumulative mean are due to secondary growth. A curve drawn through the initial score of each day coincides approximately with that drawn through the points representing cumulative mean scores of these data, but the interpretation that the cumulative mean may therefore be applied to other data as a measure of primary growth leads to mathematical consequences which Snoddy does not notice.

As an illustration of the use made of the notion that secondary growth lies above the curve of primary growth, consider the following equations (**4**, p. 35):

$$\begin{aligned} &(1)\ \text{Total growth} &&= 20.34 + 10.60 \log x \\ &(2)\ \text{Primary growth} &&= 20.34 + 4.86 \log x \\ &(3)\ \text{Secondary growth} &&= \quad 0 + 5.74 \log x \end{aligned}$$

Secondary growth is determined by subtracting the equation for primary growth from the equation representing total learning. Although the initial constant (20.34) is simply the score of the first point, through which the first two logarithmic curves are made to pass, the reduction of this constant to zero by subtraction is given a theoretical interpretation:

> It appears, then, that secondary growth has no added constant, but rests upon the base provided by primary growth (**4**, p. 36).

This faulty interpretation of a mathematical artifact as of theoretical importance is paralleled by the failure to note that the cumulative mean of a logarithmic curve bears a definite relationship to that curve. If the curve is $y = a + b \log x$, then the cumulative mean will be $y = a + 1/2\ b \log x$. This is evident if the logarithmic equation is thought of as a straight line function; the cumulative mean of points on a line is another line of half the slope. If the total growth curve above is accepted, the mathematical relations of

primary and secondary growth may be derived without a separate empirical determination of the curve for the cumulative mean:

Given, (1) Total growth $= 20.34 + 10.60 \log x$

then, (2) Primary growth $= 20.34 + 5.30 \log x$,

since $\frac{10.60}{2} = 5.30$,

and (3) Secondary growth $= 0 + 5.30 \log x$

These values do not depart very widely from the empirical ones reported by Snoddy. In other words, if the learning curve is logarithmic, if primary growth can be expressed as a cumulative mean and secondary growth found by subtraction, *secondary growth is equal to primary growth minus a constant representing the initial score.* The operational definition of primary growth as the cumulative mean forces the conclusion that, if the learning curve is logarithmic, primary and secondary growth differentials are *equal* after the first score. This conclusion is not intended by Snoddy, for it contradicts his interpretation that an experimental arrangement favorable for early primary growth cannot also be favorable for secondary growth, and conversely, that a situation favoring secondary growth cannot continue to be favorable for primary growth.[4] Since primary and secondary growth increments may turn out to be equal, if the above calculations are correct, doubt is raised about the fundamental proposition that early and late massing of practice have different effects because of the early preponderance of primary growth and the later preponderance of secondary growth.

(2) *The Supposed Favorable Effects of Massing of Trials Late in Practice.*—Snoddy shows that as practice continues in groups with different inter-trial rests, there is a slight decline in the ratio of gains made by the more favored groups (with distributed practice) over the gains made by the less favored groups (with massed practice). Thus during trials 1–5 a

[4] Although he finds primary growth to be correlated with the secondary growth above it ($r = + .77 \pm .03$), Snoddy does not accept this correlation as contradictory to the fundamental opposition of the processes (4, p. 47).

group with 1-minute rests gains 1.75 times as much as a group with continuous practice; within the much later trials 101–120 this ratio is reduced to 1.65 (4, Table III). While it is evident that even so far along in practice the group with spaced practice is gaining more than the group with massed practice, the slight reduction in the ratio of increments is sufficient to permit Snoddy to state:

> It would appear that this growth which is cutting down the lead of the long-interval group might have two elements to bring it about: lateness and continuous-practice. . . . The particular growth in the continuous-practice group which was reducing the lead of the time-interval group appears to be of the secondary type (4, p. 22).

Such an interpretation produces the faulty impression that massed practice is favorable late in training, although the evidence shows that spaced practice is advantageous even within trials 101–120. Within these trials the continuous practice group averages 26.55, as compared with the average of 41.38 for the 1-minute group. The distributed practice group is continuing to make greater gains than the continuous practice group, in spite of the greater difficulty of increments near the upper limit of scores. Spaced practice is advantageous throughout, and continuous-practice correspondingly disadvantageous, contrary to the interpretation given his data by Snoddy.

(3) *The Interference between Primary and Secondary Growth.*—While primary and secondary growth are sometimes said to be opposite processes, it is evident that their measurement by subtraction from total growth supposes them to be of like sign. Addition of secondary growth to primary growth increases scores; subtraction of secondary growth decreases total scores. An incoherence is introduced into the conception of the two processes in a section which deals with their interference (4, pp. 29–34). It appears in this section that secondary growth may actually cause a subtraction of primary growth, which contradicts both the ordinary additive principle, and also the insistence upon the stability and irreversibility of the primary growth base.

Two types of evidence are presented. The first is based on the failure of a logarithmic curve fitted to the data by

the method of least squares to pass through the first few points. Confronted with alternatives, Snoddy chooses the one less permissible mathematically:

> Either the log equation is a misfit entirely, or the two growths under conditions of continuous practice have an interference effect upon each other. . . . It seems probable that this interference effect—which is gradually overcome—in the early part of continuous-practice, is the sole cause of the poor fit of the log equation at the early stations (4, p. 29).

The second type of evidence is based on lower scores in the second of 2 continuous circuits, when the pairs of circuits are separated by 2-minute rests. The second score of each pair is lower, and hence has evidently been reduced by interference. According to the other principles espoused by Snoddy, this unfavorable effect of massing should be less later in practice, and the gains between trials (with rest) should be greater earlier in practice. His own data (4, Fig. 5), while demonstrating an interference effect, deny these other contentions by showing the deleterious effects of massing to increase as practice proceeds, while the advantage of spacing continues. His figure confirms our findings, and denies the aspects of his own theory upon which the fundamental theorem which we tested rests. The figure does undoubtedly show an interference effect, but to be consistent this should be attributed by Snoddy to some third factor different from primary and secondary growth. Unless some new factor is permitted to enter, his theory becomes internally inconsistent.

An Alternative Analysis

Snoddy's extensive experiments, participated in by over 300 subjects, furnish an important body of data. Where comparisons are possible, the data are found to be in substantial agreement with our data and with the data from other laboratories (2) (3) (5) (6). Because of the unsatisfactory nature of Snoddy's analysis, an alternative is needed.

A more conventional classification of the significant aspects of scores in skilled performances attributes improvement to learning, but recognizes that learning scores may be reduced or masked by factors producing a work-decrement. The

interferences of fatigue, refractory phase, and the like, are distinguished from a more permanent disruption which is usually called forgetting. Such a conventional analysis may be outlined as follows:

1. Learning factors.
    a. Improvement within practice (Acquisition).
    b. Loss with non-practice (Forgetting).
2. Work factors.
    a. Loss within practice (Work-decrement).
    b. Improvement with non-practice (Recovery with rest).

Comparison of such an analysis with that of Snoddy's report shows several points of agreement and disagreement. The analysis does not propose a theory of learning, but it provides a scheme of classification within which the evidence may be discussed. The chief evidence used by Snoddy in characterizing primary and secondary growth may be described somewhat as follows according to the conventional analysis:

Evidence for Primary Growth.—The unfavorable effects of massing, and the advantages of spacing, are used to infer a growth process between trials. These effects may be accounted for, however, in terms of work-decrement and recovery, so that the main features of primary growth are most closely related to what are called work factors in the outline.

The daily increase in the size of initial scores, in spite of overnight loss, provides further evidence for a base of primary growth. This daily evidence of previous acquisition does not belong with the work factors of our analysis, but with the learning factors. It means simply that overnight forgetting is incomplete, and that there is a residual gain from the previous day.

Hence Snoddy's primary growth may be said to confuse learning factors and work factors. This appears to be one of the reasons for his unsatisfactory treatment of interference, since primary growth includes interfering factors within itself. Score reductions in continuous practice and gains with rest are easily accounted for as work-decrement and recovery,

but they are difficult to describe in terms of primary and secondary growth.

Evidence for Secondary Growth.—The introduction of a second factor because continuous-practice was supposed to be advantageous late in learning was shown to contradict the evidence.

Overnight losses, however, indicate clearly some instability in the learning, particularly in the case of higher scores. Such instability of learning, described as secondary growth by Snoddy, is known conventionally as forgetting. It may be due to interference by the normal experiences during the day between sessions. It is quite possible that in the Snoddy task the normal perceptual experiences interfere with the learned inverted-vision coördination. Thus the loss attributed to the subtraction of secondary growth may rest on the very perceptual factors which Snoddy prefers to assign to primary growth (**4**, pp. 67–71).

One reason why massed practice appears to be advantageous late in learning is that relearning each day is rapid. The rapid relearning would be expected, however, because each day's practice represents that much more overlearning before the overnight forgetting. The results are coördinate with those from the more usual studies of memorization and retention.

Thus the learning factors account for the growth base, the daily increments, and the losses by forgetting. They include portions of the changes attributed by Snoddy to primary growth (*e.g.*, the growth base), and portions attributed by him to secondary growth (*e.g.*, overnight losses). The summation of primary and secondary growth to produce total growth, one of the inconsistencies in Snoddy's theory of opposed processes, causes no difficulty in the conventional analysis because the growth base and the daily increments are both positive acquisitions. Scores are reduced only by the interferences of work-decrement within practice, and by forgetting between practice sessions. Thus the conventional analysis gives a more coherent account of Snoddy's evidence than his analysis into primary and secondary growth.

The data from our experiments contradicted the predictions based on the Snoddy theorem. Would the more conventional type of analysis have predicted our results? The answer to this question must be provisional, for the conventional analysis is merely a classification. Until there is more specific knowledge about the nature of the learning going on and about the types of interference present in the massed trials, there can be no specific predictions. On the other hand, some statements may be made about our results in line with the commonplace interpretation as outlined. It is necessary to assume that learning is less stable when scores are higher, so that high scores are reduced more readily than low scores by the intra-practice interferences such as arise with massing.[5] This assumption will also account for the greater overnight losses found late in practice in Snoddy's experiments, for an unstable learning is interfered with by incidental experiences of long inter-practice periods as well as by intra-practice effects. Since Snoddy's evidence provides little which cannot be assimilated by a conventional analysis of skill, and since his own theories are refuted by experimental data, the conventional analysis is to be preferred.

Summary

An experiment was designed to test a theorem based on Snoddy's characterizations of primary and secondary growth. The theorem predicted that early spacing and later massing of practice should be more advantageous for learning than early massing and later spacing. The learning task selected for study was the visual-manual coördination of the Koerth pursuit rotor. Two groups of 25 subjects scored alike on 3 initial trials under comparable circumstances. Trials were of 1-minute duration. Group *E* practiced subsequently at intervals decreasing from 11 minutes to 1 minute, while for Group *F* the intervals increased from 1 minute to 11 minutes. Each group practiced a total of 14 trials within the 43 minutes

[5] This instability of higher scores is noted by Snoddy as one of the reasons for postulating secondary growth. Had he recognized the deleterious effects of massed practice late in learning, his account would have been more nearly in harmony with that which we are presenting.

of the practice period. With an equal number of trials, covering an equal total time, Group *F* scored reliably higher than Group *E*, contradicting the theorem based on Snoddy by showing massing to be more disadvantageous late in practice than early in practice.

A reëxamination of Snoddy's evidence showed it to be in substantial agreement with ours, and not coherent with his characterizations of primary and secondary growth. An alternative classification of the evidence in terms of learning and forgetting, on the one hand, and work-decrement and recovery, on the other, is recommended because it relates the problems within visual-manual learning to those studied in other investigations of practiced performances.

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A REPLY TO DORÉ AND HILGARD

BY GEORGE S. SNODDY

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I am grateful to the JOURNAL OF EXPERIMENTAL PSYCHOLOGY and to these authors for the opportunity to reply to the criticism of the latter directed against my researches. In these days of unusual productivity in psychological research it is indeed a pleasure for one to know that one's work is being read; and differences as to interpretation and significance of findings may be minor matters, after all. No one who breaks in any way with the more established ways of thinking, as I have done in the psychology of learning, can hope to escape criticism and considerable misunderstanding, perhaps.

In their first article of criticism (1)* these authors did not find themselves at such great variance with my position as they do in the present article. A confusion began, however, in the earlier article which is carried over to the second, and even intensified, which would lead one to think that much of the difficulty for these authors is really a matter of confusion and misunderstanding. They criticize me in their first article for presenting a learning theory which omits the forgetting factor. Actually, this would seem to me to be a colossal error, if true. These writers present this criticism despite the fact that the one central issue in my study was an argument for the existence of two types of growth increment, one of which was *permanent* and the other *temporary*. Most critics would have seen at once that the chief purpose of the temporary increment was to provide specifically for what is generally called forgetting. This criticism is not mentioned in their present article, but the omission is hardly an oversight. While it was one of the prominent features of my research to show that interpolated time would produce a gain

* Numbers refer to the numbers in the present Doré and Hilgard bibliography.

under certain conditions and a loss under other conditions, both conditions being clearly specified, these writers now propose in the present article that interpolated time always produces a gain. Under such conditions, what has become of the forgetting factor? And who is leaving it out of consideration? It may be that these writers have merely gained a new insight, which is wholly commendable, but it happens that such a finding is completely contradicted by common experience and requires no further consideration.

These writers propose a *theorem*, which is presumed to have the power of testing my principle of the two growth processes, the first or primary one, held by me to be greatly facilitated by interpolating time, and the second or later one, facilitated by withdrawing time. At the outset, if they are to be fair, they should regard this formulation as a theorem—that is, as a proposition to be proven—but, instead, they regard it as an axiom which is to be accepted as self-evident. Actually, its utility for the purpose intended is far from self-evident, and upon analysis there is revealed in it a defect which completely destroys its value.

The writers propose in their theorem two equated groups of subjects operating under different practice conditions. One group is first given conditions approaching continuous practice, or massed practice, in their phrasing, then is changed over to conditions of time-interval or distributed practice. A second group is first given conditions of time-interval practice and then is changed over to conditions of massed practice. Since, as they believe, conditions have been arranged to provide for early and late factors and the total time of practice kept constant for the two groups, then all they need to do, in their opinion, is to compare end results to determine the truth of my contentions. This entire setting is, to me, extremely strained, artificial and wholly out of accord with the controls used in my study. In the mirror-vision coördination we can actually deal with discrete repetitions of a coördination having two dimensions, time and errors. These time and error quantities remain as variables throughout the experiment. They are equated by us to facilitate the

use of a velocity score, but this in no way interferes with their variable character. We modify this natural setting by interpolating or withholding time between repetitions, as required. How different this is from the Doré and Hilgard setting where practice time is held constant for the two groups! In my setting the time of the coördination at each repetition is a variable; in their setting the time of practice is a constant, since they have no possible way of isolating the coördination and timing it. Despite this enormous shift from what is variable to what is constant, these authors propose to refute my findings, willy nilly, by holding practice time a constant.

The situation above noted is far from satisfactory for my critics. The procedure these writers propose is, however, gradually realizable very late in the learning when the time of the coördination is itself practically a constant, as demonstrated in my researches. The issue here resolves itself into the simple principle, that where time in nature is itself a variable it is playing a role and cannot be made a constant; and where time has ceased to vary we may study *attained efficiency* by making time a constant. Thus, we could not profitably study the whole learning process by making the time of performance a constant, but only late portions of it. If the learning task is young, as is the case with the Doré-Hilgard task, and such a procedure as theirs is adhered to, what one gets is a confused picture, excessively difficult to interpret.

The above is entirely general and is directed toward their theorem and their handling of the data. However, a specific defect develops in their study which reduces its effectiveness completely. The group with continuous practice early and time-interval practice later will have no difficulty and data from this group will be fairly useful. But in the case of the group first adapted to time-interval conditions and then shifted to continuous-practice conditions *in the same practice sitting*, an interference or inhibitory effect will appear which will reduce the continuous practice scores enormously. This point was carefully developed in my research (**4**, p. 29). To my knowledge, this type of interference has not been de-

veloped elsewhere, but it is of the greatest importance. If in a learning situation, we let *r* represent a repetition and a *dash* represent an interval of time, say two minutes, a practice that would be indicated by *r—r—r—rr—rr—rr*, etc., presents a marked lowering of score for the second member of the paired repetitions (**4**, p. 33, Fig. 5). It does not occur if the pairing is begun at the first repetition, or if we have five, six or any number of repetitions in continuous sequence from the beginning of practice (**4**, p. 30, equations 10, 12, 13 and p. 34, Fig. 4). The phenomenon of interference apparently rests on early adaptation to time-interval conditions with a shift, in the same practice sitting, to conditions of continuous practice. But whatever this phenomenon is, and however it be interpreted, it is an experimental fact, statistically proven, which will forever block any attempt to reveal the growth coming from continuous practice following time interval adaptation, in one practice sitting. Thus, their theorem is hopeless.

Had these authors removed the interference by interpolating an over-night time interval and then measured the relatively permanent efficiency immediately following the interval they would have found their two groups (Fig. 1, Doré and Hilgard) at the same efficiency level. The difference between these two groups is exactly equal to the interference produced by the conditions of practice. These details of interference were so carefully analyzed in the study they assail that these authors should have seen their entire research finding as merely a demonstration of the interference phenomenon previously proposed by me. A critical attitude toward their theorem would have led to other and no doubt better experimental procedures.

If to these criticisms these authors reply that I seem to be skeptical of the pursuit-rotor type of learning, my answer is an emphatic no. This type of learning is excellent because it begins at a very early level where the primary or stabilization type of growth is taking place and, unfortunately, this is not true of the vast majority of present-day learning situations. Humphreys (**2**) in his study of this same pursuit-rotor learning

directed his attention wholly to what was actually happening in the learning; that is, he sought to determine where and under what conditions the growth increments were taking place. His research, which was done elsewhere and published from our laboratory, was in complete accord with my own findings, a fact which Doré and Hilgard neglect to mention. Humphreys spread his experiment over three days of practice, avoided interference effects to a large degree and, what is highly important, drew his conclusions from sequential differences in his whole group behavior rather than from direct comparison of efficiency scorings for different groups. That is, this author studies growth or increment directly and not as a deduction from comparative, simultaneous efficiencies, as was attempted by Doré and Hilgard. The method employed by Humphreys is essentially that used in my own research, and the results are in complete accord, which puts the entire Doré and Hilgard study in a peculiar light.

Humphreys used fifty subjects over very short work periods involving 5 trials each. Intervals of 5 minutes separated the successive work periods, but the trials were in continuous sequence. Thirty trials, or six work periods, occurred on each of three days. His findings are so clear-cut and so much at variance with the Doré and Hilgard interpretations that a short table drawn from Humphreys is presented here.

TABLE I

GAINS DURING AND BETWEEN PRACTICES

During Practice Differences between first and last scores of every group of five trials			Between Practice Differences between the last trial of one group and the first trial of the next group		
1st day	2d day	3d day	1st day	2d day	3d day
14.04	12.90	5.52			
8.40	8.12	5.92	34.76	9.12	2.36
−2.32	1.66	8.98	16.76	−3.28	−6.98
−3.64	−5.06	11.06	21.18	10.86	−4.36
−6.86	−1.96	1.50	11.62	6.62	−3.62
−8.74	−5.56	3.06	21.68	9.30	2.64
Σ .88	10.10	36.04	106.00	32.62	−9.96
Av. .15	1.68	6.01	21.20	6.52	−1.99

By reference to the averages at the bottom of the table it will be seen that the gains from continuous effort, that is, during practice, are very much greater late than early; while the gains over the interval, that is, between practice, are very much greater early than late. In other words, the time interval is having its positive effect early in the learning, while late in the learning the gain is coming largely from continuous effort. These, of course, are precisely the findings reported by me for the mirror-vision coördination and no further comment is necessary.

A retrospective glance at the Doré and Hilgard study impresses me with the notion that their somewhat petulant attitude and confused attack grow out of my interpretation of the role of interpolated time as an early stabilizing factor, rather than as an eliminator of fatigue and related work-effects. But these writers are perhaps not alone in this. Despite the fact that it has been widely current for years that the elimination of fatigue cannot of itself produce a true learning increment, but only a return to a former state, there are many instances where writers still fall into confusion on this point.

In my writings the greatest pains have been taken to show that when growth increments for time-interval versus continuous-practice conditions are compared, the superiority of the time-interval conditions is confined wholly to the early part of the practice interval. In other words, the increments in continuous practice are low only early in the continuous practice. They gain on the time-interval increments from beginning to end of any normal, continuous-practice interval. How can anyone attribute these effects to fatigue, when the greatest lag for the continuous-practice group is very early and their greatest gain in relative increment is late in the continuous practice? The relations here are wholly reversed from those required if an accumulating fatigue were producing the lag. It does not appear that the complete break-down of the fatigue explanation in this situation involves any doubt whatsoever. Learning increment as an elimination of fatigue or work-effects must be discarded com-

pletely, as has long been the case in critical learning theory. Because of this, the role of time in learning cannot possibly be conceived as in any sense an eliminator of fatigue, although this function no doubt occurs on occasion, but under such conditions as to make the process appear always as independent of the learning itself.

In my Figs. 1 and 2 are presented two very simple illustrations of the situation just discussed. These data are

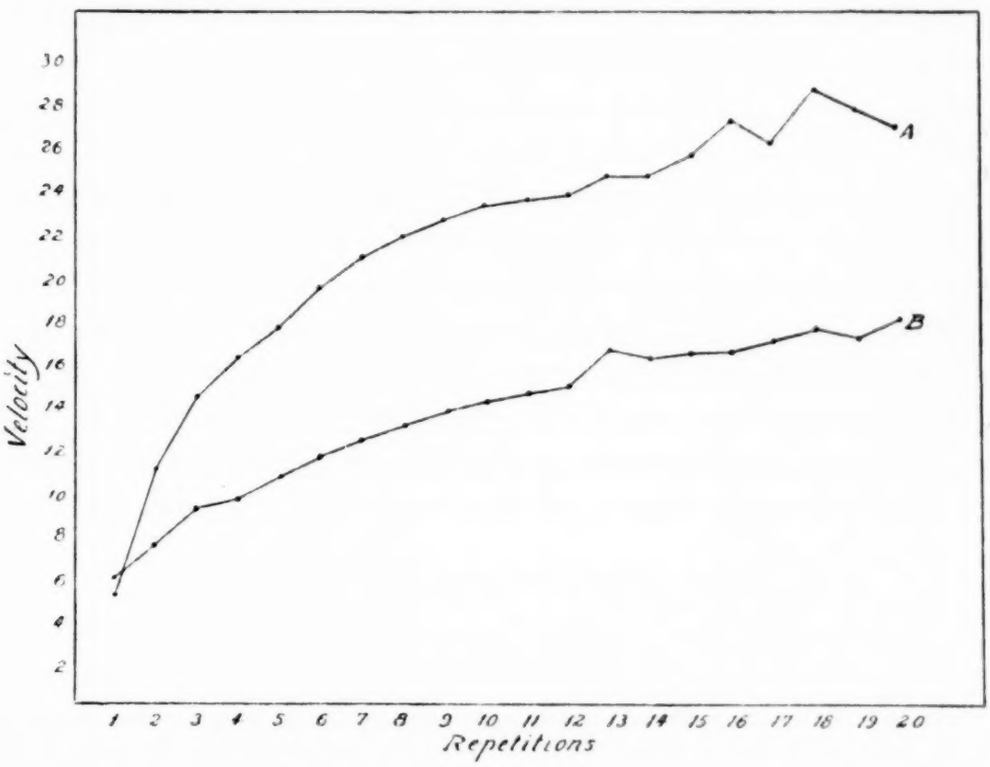

FIG. 1. This shows two learning curves drawn from the mirror-vision coördination. In *A* there was one repetition per day, while in *B* there was continuous practice.

drawn from the mirror-vision coördination which is under criticism. The scores in Fig. 1 are average velocities and each group contains 70 subjects. The group marked A was the time-interval group, while the group marked B operated under continuous-practice conditions. In Fig. 1 the time-interval group had twenty-four intervals between successive practices of one repetition each.

Figure 2 was prepared in the following way. The initial score was subtracted from the entire sequence of scores as

shown for each group in Fig. 1. This gave us the increments of growth for each group under each type of practice. Fig. 2 shows the ratio of the continuous-practice increments to those of the time-interval practice. Thus we are comparing increments from a situation where fatigue is presumed to exist (continuous-practice) with a time-interval situation (one repetition per day) where fatigue could not be presumed at all. The results leave nothing to be desired in showing

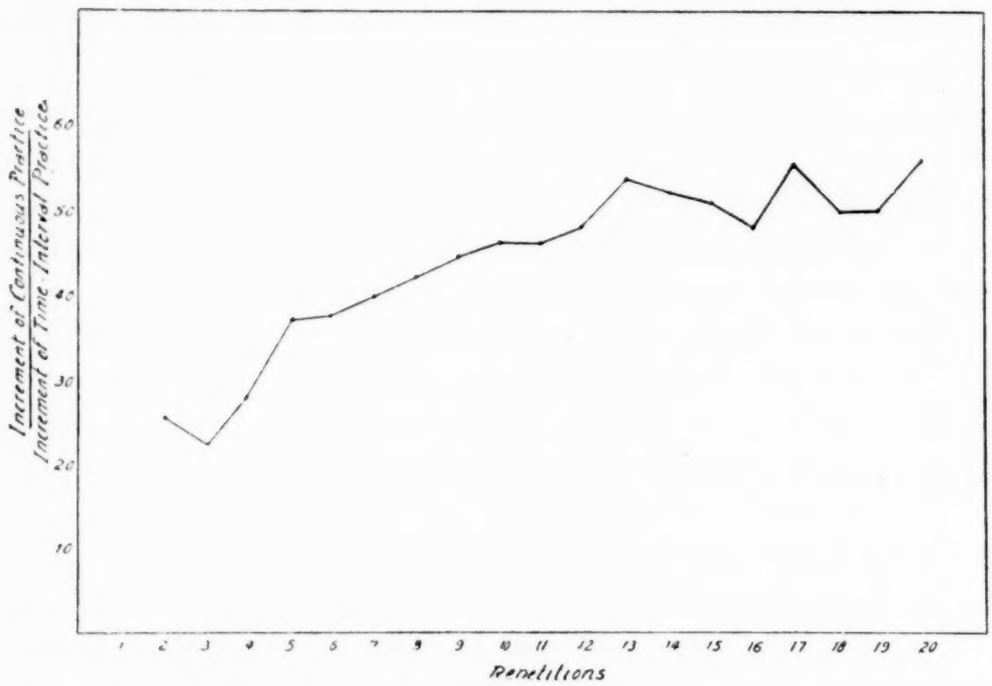
Increment of Continuous Practice / Increment of Time Interval Practice
60
50
40
30
20
10
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
Repetitions

FIG. 2. This comparison of growth increments was made from the data shown in Fig. 1. The initial score is subtracted out to get the increment, then the ratio of the increment in continuous practice to that in time-interval practice is found and plotted. The ratio of increments would begin with the second repetition. The scores on the vertical axis are percentages.

that the enormous advantage of the time-interval cannot be due to the elimination of fatigue. My own interpretation here was that early in the history of this coördination the stimulus-situation itself is acting as a shock; it produces a state of disequilibrium in the organism which the organism begins at once to repair. The repair begun by the organism is essentially a process of equilibration which takes place during stimulation or without stimulation in exactly the same direction. If time is interpolated here the process of equilibration is carried forward or enhanced, but if time is admitted only

grudgingly, as in continuous practice, the total equilibration is not so great, as shown in Fig. 1.

Many processes in the physical world, such as settling, present this tendency to complete equilibration across time. If we shake a molecular mass, the material settles, but if we cease shaking, the process of moving toward a more complete equilibrium is continued across the interval, however long it may be; thus the process of equilibration always presents this completing tendency throughout nature. From these considerations, the role of time in learning is seen to be not a matter of the removal of fatigue-effects, but is instead a process of stabilization which carries always a forward reference, that is, it is basic for other processes to come. From this view the organism is observed to develop its own control in a completely naturalistic setting. However, insight into such matters has apparently escaped my critics entirely.

I pass over without comment the greatly confused picture of the quantitative aspects of the Doré and Hilgard study. The details here are difficult for those who have time to study them in my own research or that of Doré and Hilgard, but for others, a criticism would be largely a waste of time.

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INCREASE IN FREQUENCY OF THE ALPHA RHYTHM BY VERBAL STIMULATION

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The most spectacular brain potential rhythm is the alpha or Berger which is best seen from the occiput of the human being under conditions of minimal stimulation. It has a frequency of about 10 per sec. and the waves may be nearly sinusoidal and of 30 to 100 microvolts in intensity. Under various conditions of stimulation this rhythm can be greatly altered. It may be obliterated entirely or broken-up into faster, smaller and more irregular oscillations. This break-up is not a gradual process in which the alpha waves increase continuously in frequency and decrease continuously in height, but rather it consists of an immediate shift from a 10 per sec. high amplitude to a much faster and low amplitude rhythm. In the present study, we were not interested in such changes in brain potential patterning but in continuous changes in the frequency of the dominant alpha rhythm without appreciable changes in amplitude or patterning.

Hoagland (3) found a continuous increase in the frequency of the alpha waves under conditions of increased body temperature. In children, Berger (2) and Lindsley (4) found a progressive increase with age. These results were interpreted as due to physiological changes not brought about by immediate afferent stimulation. The purpose of the present study was to determine whether or not a continuous increase in the frequency of the alpha rhythm could be produced by immediate afferent stimulation.

Procedure

Throughout an entire experiment the subject reclined in a dark and partially sound-proofed room with his eyes closed. Under conditions of minimal stimulation (control) he was instructed to relax and maintain a passive attitude. The control periods preceded and followed the periods of stimulation. During the stimulation

periods, material was read to the subject through an inter-communicating system. He was instructed to attend closely to the reading. The material was chosen with the expectation that different types would have differential effects. Thus, it was anticipated that abstract material taken from Plato would have less effect than descriptive material chosen to elicit various types of imagery or affective experiences. Preliminary study showed no differences in effectiveness of the type of material upon the frequency of the alpha waves. Therefore, in the results all reading passages were considered together. About one-third of each experimental session which lasted approximately an hour was devoted to reading. During both the control and stimulation periods the soft hum of the undulator's motor could be heard by the subject.

The brain potentials were recorded with one electrode on the occiput and the other on the lobe of one ear.

Eight men and four women served in the study. The possession of a dominant alpha rhythm was the main criterion for the choice of these subjects.[1]

Results

In the evaluation of the records it is to be emphasized that all frequencies reported were measured from successive seconds in a clear train of alpha waves. Furthermore, alpha waves were strictly limited to those ranging in duration from 83 to 125 milliseconds (8 to 12 per sec.). In a series of regularly repeated alpha waves of several seconds duration the number of waves appearing in each second was counted. Values were expressed in terms of complete waves. In Fig. 1,

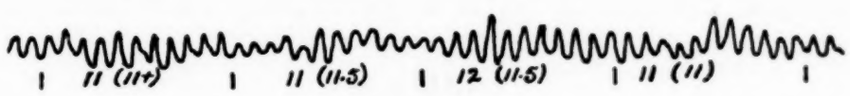

Fig. 1. Record illustrating the method of reading the frequency of the alpha waves. Seconds marked off at bottom of record. Two frequency readings are given for each second. The one in parentheses is the more exact, second by second. In order to avoid fractions the other one was used. In either instance the total number of waves is 45 and the mean frequency, 11.25.

the first second was read as containing 11, the second 11, the third 12 and the fourth 11 waves; whereas, the first second actually contained 11 and a fraction, the second approximately 11.5, the third approximately 11.5, and the fourth 11 waves. In either case the total number is 45 waves and the mean frequency, 11.25. This technique resulted in the summation of fractions of waves, not a fraction of a wave being discarded. However, it increased the variability in

[1] In selecting only subjects belonging to the dominant alpha group we are cognizant of the possibility of introducing a sampling error.

frequency which in turn decreased the reliability of the differences obtained.

The reliability of reading frequencies was determined as follows: The two authors read independently of each other the same strip of record. No markings whatsoever were made on the record by the first author reading the frequencies so that the second author had no knowledge of exactly what seconds had been read. L. E. T. read 114 seconds to obtain a mean of 9.72 waves per sec. Twenty-nine and eight-tenths percent of his seconds were read to give nine waves per sec.; 68.5 percent, 10 per sec.; and 1.7 percent, 11 per sec. J. P. E. read 100 seconds to obtain a mean of 9.74 waves per sec. Twenty-eight percent of his seconds were read to give nine waves per sec.; 70 percent, 10 per sec.; and 2 percent, 11 per sec. These results indicate that frequency readings may have a high degree of objectivity.

The statistically significant increase in the frequency of the alpha rhythm during verbal auditory stimulation over that during conditions of minimal stimulation (control periods) is the essential finding of this study. Table I shows this to be true of each of the ten subjects when the listening period was compared with a preceding 10-minute control

TABLE I

THE FREQUENCY OF THE ALPHA WAVES IN A PRECEDING TEN-MINUTE CONTROL AND IN A VERBAL AUDITORY STIMULATION PERIOD

Subjects	Control				Stimulation				M_2-M_1	CR
	M_1	R	S.D.	N*	M_2	R	S.D.	N		
I	10.53	10–12	.52	150	11.13	10–12	.49	210	.60	11.1
II	10.10	8–12	.91	100	11.00	9–12	.64	260	.90	9.1
III	9.46	9–10	.50	75	9.95	9–11	.34	296	.49	8.1
IV	9.96	8–11	.59	72	10.42	8–12	.61	345	.46	6.1
V	10.70	9–12	.56	150	11.07	10–12	.32	132	.37	6.9
VI	10.36	9–11	.60	150	11.09	10–12	.42	80	.73	10.7
VII	10.01	9–11	.36	100	10.41	10–11	.49	64	.40	5.6
VIII	10.63	9–12	.58	100	11.41	10–12	.53	100	.78	10.0
IX	10.28	9–11	.49	100	10.76	9–12	.54	71	.48	5.9
X	9.45	9–10	.50	100	9.91	9–11	.32	81	.46	7.5
Mean	10.15				10.72				.57	

* In all tables the N is the number of seconds.

period. Tables II and III show this same result in terms of percentage differences for a particular frequency. In Table III it is to be noted that the lower frequencies decreased and the higher frequencies increased in percentage of occurrence during the stimulation period.

TABLE II

PERCENT OF SECONDS GIVING A CERTAIN FREQUENCY FOR THE TEN-MINUTE CONTROL AND THE VERBAL AUDITORY STIMULATION PERIOD

Subject	Control						Stimulation					
	Waves per Second						Waves per Second					
	8	9	10	11	12	N	8	9	10	11	12	N
I			48	51	1	150			6	74	20	210
II	6	16	47	27	4	100		1	19	61	19	260
III		53	47			75		10	88	2		296
IV	3	11	74	12		72	1	2	57	37	3	345
V		3	27	67	3	150			2	89	9	132
VI		7	50	43		150			5	81	14	80
VII		6	87	7		100			59	41		64
VIII		4	30	65	1	100			2	55	43	100
IX		2	68	30		100		2	25	69	4	71
X		55	45			100		10	89	1		81
Mean	0.9	15.7	52.3	30.2	0.9		0.1	2.5	35.2	51.0	11.2	
				Total N = 1097						Total N = 1639		

TABLE III

CR's FOR PERCENTAGE DIFFERENCES IN TABLE II

	Waves per Second				
	8	9	10	11	12
Stimulation mean percentage (N = 1639)	0.1	2.5	35.2	51.0	11.2
Control mean percentage (N = 1097)	0.9	15.7	52.3	30.2	.9
Diff.	−0.8	−13.2	−17.1	20.8	10.3
CR		10.1	8.5	10.9	11.4

The possibility that a change in the mental condition of the subject with the mere passage of time was responsible for the increased frequency of the alpha rhythm during the listening period was tested with seven subjects (Tables IV and V). In the thirty-minute control period the frequency of the waves increased significantly only during the first twenty

TABLE IV

CHANGE OF FREQUENCY WITH THE PASSAGE OF TIME (30 MINS.) UNDER CONDITIONS OF MINIMAL EXTERNAL STIMULATION

Subject	First Ten Mins.				Second Ten Mins.				Third Ten Mins.			
	M_1	R	S.D.	N	M_2	R	S.D.	N	M_3	R	S.D.	N
I	9.94	9–11	.37	100	10.38	10–11	.48	100	10.38	9–12	.50	100
II	10.08	9–11	.58	100	10.11	9–11	.49	100	9.70	9–11	.49	84
III	9.84	9–10	.37	100	10.09	10–11	.28	57	10.12	9–11	.43	100
IV	10.97	10–12	.39	100	11.24	11–12	.43	100	10.97	10–12	.36	100
V	9.74	9–11	.48	122	10.26	9–11	.46	100	10.25	9–11	.50	100
VI	10.28	9–11	.49	100	10.62	10–11	.48	66	10.56	9–12	.57	100
VII	9.45	9–10	.50	100	9.51	9–10	.50	64	9.54	9–10	.49	83
Mean	10.04				10.31				10.21			
	M_2-M_1		CR		M_3-M_1		CR		M_3-M_2		CR	
I	.44		7.2		.44		7.0		.00			
II	.03		0.4		−.38		4.9		−.41		5.7	
III	.25		4.8		.28		4.9		.03			
IV	.27		4.7		.00				−.27		4.8	
V	.52		8.2		.51		7.7		.01			
VI	.34		4.4		.28		3.7		−.06			
VII	.06		0.8		.09		1.2		.03			
Mean	.27				.17				−.10			

TABLE V

COMPARISON OF THE STIMULATION PERIOD WITH THE PRECEDING TEN-MINUTE CONTROL PERIODS AND WITH THE TEN-MINUTE POST-STIMULATION PERIOD

Subject	Stimulation				Post-Stimulation			
	M_4	R	S.D.	N	M_5	R	S.D.	N
I	10.73	10–12	.46	100	10.37	9–11	.58	122
II	10.24	10–11	.43	100	9.82	9–11	.47	81
III	10.21	9–11	.45	100	10.00	9–11	.40	50
IV	11.26	11–12	.44	100	11.10	10–12	.48	100
V	10.50	10–11	.50	100	10.16	9–11	.39	100
VI	10.76	9–12	.54	71				
VII	9.91	9–11	.32	81				
Mean	10.51 (10.59 for 1st 5 cases)				10.29			
	M_4-M_1*	CR	M_4-M_2*	CR	M_4-M_3*	CR	M_4-M_5	CR
I	.79	13.4	.35	5.3	.35	5.1	.36	5.2
II	.16	2.2	.13	2.0	.54	7.9	.42	6.3
III	.37	6.4	.12	2.0	.09	1.5	.21	2.9
IV	.29	5.0	.02		.29	5.1	.16	2.5
V	.76	11.5	.24	3.5	.25	3.5	.34	5.4
VI	.48	5.9	.14	1.6	.20	2.3		
VII	.46	7.5	.40	5.6	.37	5.8		
Mean	.47		.20		.30		.30	

* M_1, M_2 and M_3 taken from Table IV.

minutes for five of the seven subjects. The third ten-minute period as compared with the second showed no significant changes in frequency for five subjects. Two subjects gave a significant reduction in frequency.

The listening period gave a significant increase in the frequency of the alpha waves over the first ten-minute period in 6 of 7 subjects; over the second ten-minute period in 3 of 7 subjects; over the third in 5 of 7 subjects; and over the post-listening period in 4 of 5 subjects (Table V). No subject in any of these control periods gave a mean frequency as great as that for his listening period. In general the frequency of the alpha rhythm increased with the passage of a certain amount of time (20 minutes) under conditions of minimal external stimulation with a still further increase under conditions of verbal auditory stimulation (Fig. 2). Further evidence of

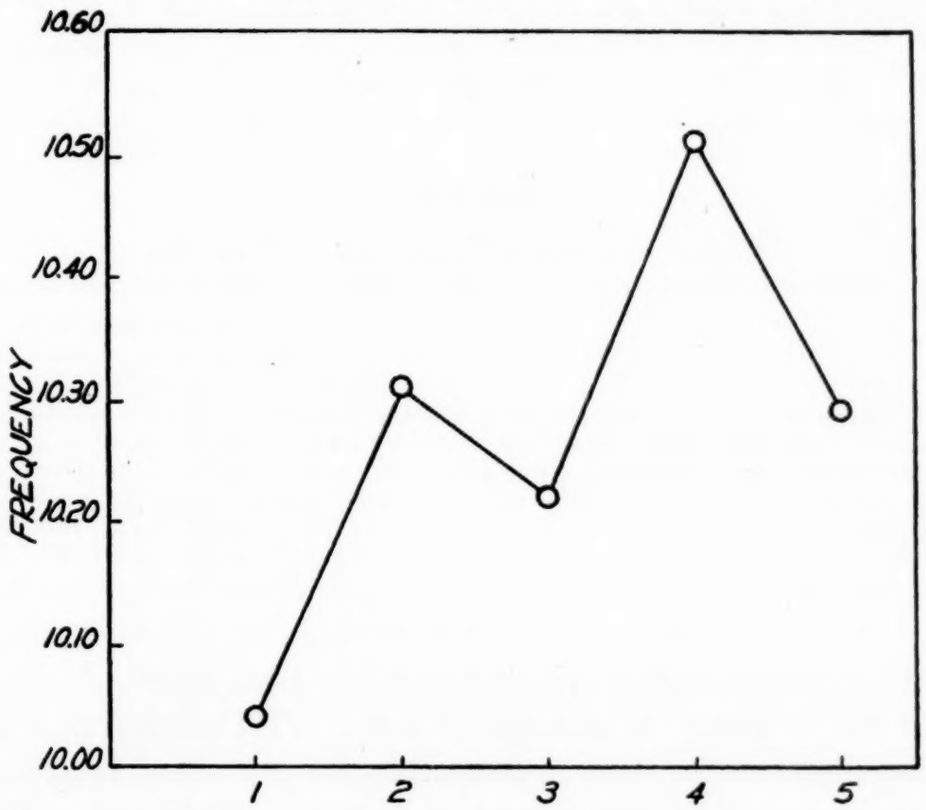

FIG. 2. Graph showing the changes in frequency of the alpha waves with the passage of time under conditions of minimal external stimulation (pre-stimulation, 30-minute control period), with verbal stimulation, and with cessation of verbal stimulation (post-stimulation, 10-minute control period). The ordinate scale gives the mean frequencies for the group of seven subjects (Tables IV and V). On the abscissa, 1 = 1st ten-minute, 2 = 2nd ten-minute, 3 = 3rd ten-minute, 4 = verbal stimulation, and 5 = post-stimuation period.

this general finding is provided by Tables VI and VII which show percentage differences for a particular frequency for the listening and post-listening periods.

TABLE VI

PERCENT OF SECONDS GIVING A CERTAIN FREQUENCY FOR THE STIMULATION AND THE POST-STIMULATION PERIOD

Subject	Stimulation					Post-Stimulation				
	Waves per Second					Waves per Second				
	9	10	11	12	N	9	10	11	12	N
I		28	71	1	100	5	52	43		122
II		76	24		100	21	75	4		81
III	2	75	23		100	8	84	8		50
IV			74	26	100		7	76	17	100
V		50	50		100	1	82	17		100
Mean	0.4	45.8	48.4	5.4		7.0	60.0	29.6	3.4	
			Total N = 500					Total N = 453		

TABLE VII

CR's FOR PERCENTAGE DIFFERENCES IN TABLE VI

	Waves per Second			
	9	10	11	12
Stimulation mean percentage (N = 500)	0.4	45.8	48.4	5.4
Post-stimulation mean percentage (N = 453)	7.0	60.0	29.6	3.4
Diff.	−6.6	−14.2	18.8	2.0
CR	4.7	4.4	5.9	1.5

With four subjects relatively short silent intervals separated the reading of passages. Table VIII shows that all subjects gave a decrease in frequency for the silent intervals. For only two, however, was the decrease significant. This table shows also that for all the subjects the frequency for the inter-passage periods was significantly greater than that for the ten-minute control period before verbal stimulation. The data for one subject are presented graphically in Fig. 3.

TABLE VIII

COMPARISONS BETWEEN CONTROL, LISTENING AND INTER-PASSAGE PERIODS

Subject	Control				Stimulation				Inter-passage			
	M_1	R	S.D.	N	M_2	R	S.D.	N	M_3	R	S.D.	N
I.....	9.46	9–10	.50	75	9.95	9–11	.34	296	9.71	9–11	.36	323
II.....	9.96	8–11	.59	72	10.42	8–12	.61	345	10.22	9–11	.48	335
III.....	10.53	10–12	.52	150	11.13	10–12	.49	210	11.12	10–12	.39	286
IV.....	10.10	8–12	.91	100	11.00	9–12	.64	260	10.86	9–12	.54	219
Mean	10.01				10.63				10.48			
	M_2-M_3		CR		M_3-M_1		CR		M_2-M_1		CR	
I.....	.24		8.7		.25		4.1		.49		8.1	
II.....	.20		4.9		.26		3.6		.46		6.1	
III.....	.01				.59		12.3		.60		11.1	
IV.....	.14		2.6		.76		7.8		.90		9.1	
Mean	.15				.47				.62			

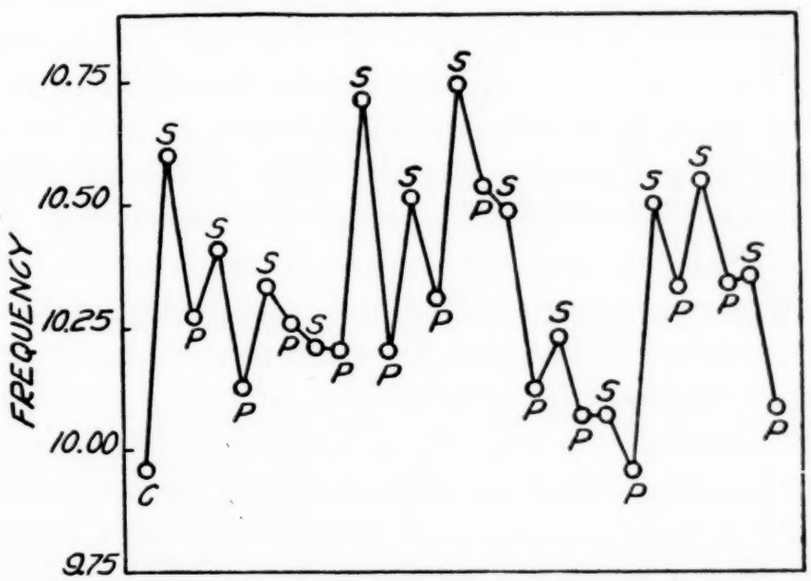

FIG. 3. Graph showing the frequencies of the alpha waves during periods of listening to the reading of passages and during the inter-passage periods for one subject. *C* = control period before verbal stimulation, *S* = periods of listening and *P* = inter-passage periods.

Since three subjects served under similar conditions of minimal external stimulation on two different occasions the variability in the frequency of the alpha waves from time to time could be determined. Table IX shows that two of the subjects varied significantly.

TABLE IX

VARIABILITY IN FREQUENCY FROM TIME TO TIME FOR THE SAME SUBJECT

Subject	First Day				Second Day				M_2-M_1	CR
	M_1	R	S.D.	N	M_2	R	S.D.	N		
I	9.46	9–10	.50	75	9.74	9–11	.46	100	.28	3.8
II	9.94	9–11	.37	100	9.96	8–11	.59	72	.02	
III	10.63	9–12	.58	100	10.97	10–12	.39	100	.34	4.9

DISCUSSION

Since one cannot specify the position of the potential sources of the alpha rhythm, one cannot distribute the magnitude among the sources. From this it follows that changes in frequency cannot be exclusively attributed to any one of three conditions; changes in the rate of the synchronized discharges of a group of cells, a break-up of such collective action, or bringing in new discharging units. Regardless of the mechanism of production of changes in frequency, it appears that the alpha rhythm can be increased in rate as a result of immediate afferent stimulation as well as by the physiological processes correlated with increased body temperature and maturational changes in the central nervous system. Further, the mechanism through which auditory stimulation operates to increase the frequency of the alpha waves continues to function for some time at least, although in most instances to a lesser degree, after the withdrawal of such stimulation. It may be conjectured that this mechanism perseverates by virtue of internal afferent stimulation or continued mental activity. If Hoagland is to interpret the increase in the frequency of the alpha rhythm solely on the basis of increased body temperature producing a shift to different pace-maker reactions, he may be overlooking this factor of internal afferent stimulation, since it is well known that increased body temperature results in increased tension and discomfort.[2] Furthermore, the concept of a chemical

[2] Although no measurements were made to determine possible changes in body temperature of our subjects during the stimulation periods, it is certainly safe to assume that such changes, if they did occur, could not have been of sufficient magnitude to produce tension and discomfort.

pace-maker is independent of the μ-values of the system involved (1).

The interpretation advanced by us that increased afferent stimulation results in increased frequency is supported by the introspective reports of the subjects that as the thirty-minute control period progressed, their mental processes changed from a generalized, low state of activity to a more restricted and active train of thought. In most subjects, paralleling this increased channeling of thought there was a decrease in the number of relatively long trains of alpha waves.

Summary and Conclusion

During periods of verbal auditory stimulation as compared with periods of minimal external stimulation the frequency of the alpha rhythm increased significantly (.57 waves per sec.). In a thirty-minute control period (minimal external stimulation) the frequency of the alpha waves increased significantly during the first twenty minutes for the majority of subjects. During the last ten minutes no further increase was found.

An analysis of the conditions of our experiment supports the conclusion that immediate afferent stimulation or increased mental activity may increase the frequency of the alpha rhythm.

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FORMULAS FOR VISUAL DISTANCE AND SIZE; THEIR RELATIONSHIP TO THE NERNST–HILL THEORY OF NERVOUS EXCITATION *

BY H. SPOTNITZ, M.D.

In recent papers (1) Elsberg and Spotnitz have presented formulas for the visual threshold at the fovea and for light and dark adaptation. The studies led to the view that cube root relationships might be representative of volume effects, and square root relationships of surface effects on the linear activity of the central nervous system. An attempt has been made to apply these principles to the results of other quantitative studies of visual function. In this report results of such studies of previously published data on visual distance and visual size are presented and their possible relationship to the Nernst-Hill theory of nervous excitation is considered.

1. Formulas for Visual Distance

Several years ago the writer made quantitative measurements of visual distance in the laboratory of Professor M. H. Fischer. These results were published in 1934 (2). A detailed description of the apparatus used was given in 1935 (3).

For present purposes it will suffice to state that two plates with concentric translucent black and white circles were placed at a measured distance from the eyes, one before each eye. The distance (B) separating the centers of each of the two plates could be regulated by means of a screw adjustment. The head of the observer was fixed by a mouth-bit. The observer looked through the plates into the distance at a white screen. After some practice it was possible to fuse the two plates and obtain an apparent image somewhat larger than either of the two plates and somewhat farther away. The distance (C) of the imaginary fused image from the eyes could be measured by running an upright black stick along a track several meters in length. The observer could control the position of the black stick by turning a hand crank. The distance of the apparent image from the eyes (C), was measured as (1) the distance of

* From the Clinical Research Laboratories of the Neurological Institute of New York. Work done under a grant for brain tumor research from the John and Mary R. Markle Foundation.

each plate from the eye (*A*) was increased, and (2) as the distance separating the centers of each to the two plates (*B*), was increased (see Fig. 1).

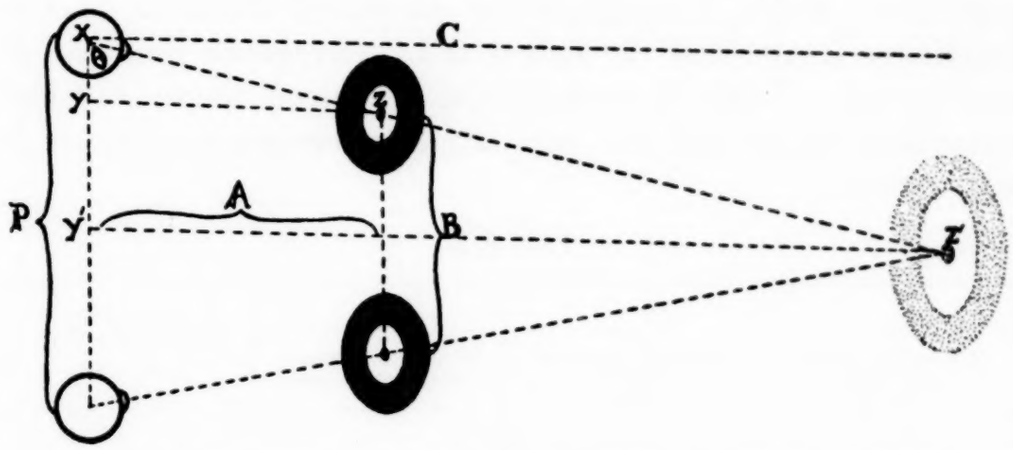

FIG. 1. Measurements used in the study of visual distance and visual size. *A* represents the distance of each plate from the eye; *B* the distance separating the centers of each of the two plates; *C* the measured distance of the apparent image from the eyes. Other letters are referred to in the text.

It is a simple problem to calculate the convergence point of the eyes during these experiments.

If A is the distance separating the plates from the eyes

P is the interpupillary distance

B is the distance separating the centers of the two plates

C is the distance of the imaginary image from the eye

then in the triangle xyz

$$\text{the tangent of angle } \theta = \frac{A}{1/2\ P - 1/2\ B}$$

and in the triangle $x\ y'\ z'$

$$\text{the tangent of angle } \theta = \frac{C}{1/2\ P},$$

$$\text{then } \frac{A}{1/2\ P - 1/2\ B} = \frac{C}{1/2\ P}$$

$$\text{or} \qquad C = \frac{A}{1 - (1/P)(B)}$$

The calculated convergence point of the two eyes was found to be about the same as the distance of the imaginary image from the two eyes which was measured during the experiments (Fischer).

When the distance separating the two plates approached the interpupillary distance, the measurements were less accurate. Table I contains the measured distance of the imaginary image and the calculated convergence point as A was varied. Table II contains the measured distance of the imaginary image and the calculated convergence point as B was varied.

TABLE I

A Distance of Plates from Eyes (cm.)	B Distance between Plates (cm.)	C Measured Distance of Unreal Image	Calculated Convergence Point $C = \frac{A}{1 - \frac{B}{6.5}}$
20	5.0	80	87
30	5.0	126	130
40	5.0	172	173
50	5.0	204	217
60	5.0	267	260
70	5.0	310	303
80	5.0	352	346
90	5.0	390	390

TABLE II (*a*)

A Distance of Plates from Eyes (cm.)	B Distance between Plates (cm.)	C Measured Distance of Unreal Image	Calculated Convergence Point $C = \frac{A}{1 - \frac{B}{6.5}}$
80	4.0	208	208
80	4.2	226	227
80	4.4	250	248
80	4.6	278	273
80	4.8	310	307
80	5.0	352	347
80	5.2	400	400
80	5.4	466	473

In view of the finding that cube root and square root relationships were of considerable significance for visual function, these measurements were studied for any such possible relationships. It was found that the formula

$$\frac{B\sqrt[3]{A}}{\sqrt[3]{C}} = \text{Constant}$$

TABLE II (*b*)

A Distance of Plates from Eyes (cm.)	*B* Distance between Plates (cm.)	*C* Measured Distance of Unreal Image	Calculated Convergence Point $C = \frac{A}{1 - \frac{B}{6.5}}$
30	4.0	77	78
30	4.2	84	85
30	4.4	92	93
30	4.6	100	103
30	4.8	112	115
30	5.0	126	130
30	5.2	146	151
30	5.4	174	177
30	5.6	212	217
30	5.8	270	278

TABLE II (*c*)

A Distance of Plates from Eyes (cm.)	*B* Distance between Plates (cm.)	*C* Measured Distance of Unreal Image	Calculated Convergence Point $C = \frac{A}{1 - \frac{B}{6.6^*}}$
50	4.2	134	136
50	4.4	146	148
50	4.6	164	162
50	4.8	184	181
50	5.0	204	203
50	5.2	230	232
50	5.4	270	271
50	5.6	320	325

* The measured interpupillary distance was found to be about 6.5 to 6.6 cm. For this table which was determined by the average of a large number of experiments extending over a period of more than a year, the more accurate coincidence with experimental results was given by an assumption of an interpupillary distance of 6.6 cm. For all other experiments accurate calculations could be made assuming an interpupillary distance of 6.5 cm.

gave a fair constant for the factors influencing visual distance. The cube root of the measured distance of the unreal fused image increased as the cube root of the distance of the two real images from the eyes, and as the first root of the distance separating the two real images. The constant did not permit as accurate predictions of the measured visual distance as was possible in general from the convergence formula. The cube root constant revealed a regular tendency to increase and to decrease which was found during numerous

calculations. However there was a sufficient coincidence and regularity of the relationships to suggest that cube root relationships are of significance as approximations for visual distance perception (see Tables III and IV).

TABLE III

A Distance of Plates from Eyes (cm.)	*B* Distance between Plates (cm.)	*C* Measured Distance of Unreal Image	Constant $\frac{B\sqrt[3]{A}}{\sqrt[3]{C}}$	*C* Calculated for Constant 308
20	5.0	80	314	85
30	5.0	126	310	128
40	5.0	172	308	173
50	5.0	204	313	212
60	5.0	267	303	257
70	5.0	310	304	301
80	5.0	352	305	343
90	5.0	390	307	387

TABLE IV

A Distance of Plates from Eyes (cm.)	*B* Distance between Plates (cm.)	*C* Observed Distance of Unreal Image	Constant $\frac{B\sqrt[3]{A}}{\sqrt[3]{C}}$	Calculated Visual Distance Constant 309
50	4.2	134	303	127
50	4.4	146	307	145
50	4.6	164	311	170
50	4.8	184	312	188
50	5.0	204	313	212
50	5.2	230	312	238
50	5.4	270	312	268
50	5.6	320	303	302

2. The Relationship of the Convergence Point of the Eyes to the Nernst–Hill Theory of Nervous Excitation

Nernst (**4**) from a study of the electrical excitation of nervous tissue and of the theoretical factors involved in ionic activity formulated the "square root of the time" equation for the excitation of nervous tissue by an electric current. Hill (**5**) further developed the Nernst ionic theory and introduced the formula

(1) $$i = \frac{\lambda}{1 - \mu\theta^t}$$

There is evidently a remarkable similarity between this equation and the equation for the convergence point of the two eyes as already developed above.

$$C = \frac{A}{1 - (1/P)(B)} \tag{2}$$

Further consideration of the two equations has indicated that the similarity between them is more than superficial. It will be recalled that in Equation 2, C represents the imaginary visual distance dependent upon some cerebral process resulting from the fusion of the message from each eye. In Equation 1, i represents the smallest current which will excite the nerve fiber under the studied conditions.

In Equation 2, A represents the distance of the real images from the eyes. In Equation 1, λ represents the smallest current which will excite at all. As stated by Keith Lucas "with very large values of t, in fact $1 - \mu\theta^t$ reduces to one, and $i = \lambda$." The same fact applies to Equation 2, for when B becomes zero (as only one plate is viewed), $C = A$.

In Equation 2, B represents the distance separating the two real images. In Equation 1, μ has the following significance in the words of Lucas (**6**), "μ has reference only to the position (relative to the membrane) at which the changes of concentration are being considered."

In Equation 2, $1/P$ represents the *reciprocal of the interpupillary* distance which is a constant for each observer during vision of a distant object. θ in Equation 1, is a function of what Hill and Lucas called k/a^2. k is the diffusion constant of the functioning ion. a is the *distance between the membranes under consideration.*

It is fairly evident from the above statements that Equations 1 and 2 are similar in more than the form of their appearance. Each variable in the one equation appears to have a parallel one in the other equation. It is possible that the two equations are indicative of the same process viewed from different aspects. The Nernst-Hill equation may be applicable to the intracerebral nervous excitation caused by the convergence of the eyes during the perception of visual dis-

tance; the convergence equation may be applicable to the parallel physical relationships in the external world—namely the distance of the actual convergence point of the eyes.

The parallelism between the Nernst-Hill equation and the convergence equation suggests that the Nernst-Hill theory of nervous excitation may give an accurate picture of the basic excitatory state during the perception of visual distance. The relationship of i, representing the exciting electric current in the Nernst-Hill equation, to C, the perceived visual distance, in the convergence equation, introduces the possibility that an electrical process based on ionic activity at surface membranes may be responsible for the perception of visual distance.

A Formula for Visual Size

Using the method outlined above (see page 394), Fischer (7) also made a study of the increase in size of the imaginary image as the distance apart of the two real images was increased. In this investigation of his results, it was found that the distance apart of the two real images (B) divided by the cube root of the size of the imaginary images (S) gave a fair constant within the range of accurate experimentation.

$$\frac{B}{\sqrt[3]{S}} = \text{Constant}$$

It was possible from the average experimental constant to predict with fair accuracy the visual size measured during the experiments (see Table V).

TABLE V

B Distance Separating Two Real Images (cm.)	S Size of Unreal Image (cm.)	Constant $\frac{B}{\sqrt[3]{S}}$	Calculated Size for Constant 199
4.2	10	196	9.5
4.4	11	198	10.8
4.6	13	196	12.5
4.8	14	199	14.0
5.0	15	203	16.0
5.2	16	207	18.0
5.4	20	200	20.1
5.6	22	200	22.4
5.8	27	194	24.8

As reported by Fischer and as may be seen from the above equation for visual size, visual size has a linear relationship to visual distance. Thus it may be calculated either from the cube root or the convergence equations.

The above formula for visual size is further evidence in support of the view that cube root relationships are of basic significance for the activity of the central nervous system and that they may be indicative of volume or space impressions in terms of the linear activity of the central nervous system.

Summary and Conclusions

Measurements of visual distance are found to coincide with the calculated convergence point of the two eyes. Visual distance may also be predicted with a fair degree of accuracy by cube root equations. Similarly variations of visual size with visual distance may be expressed by a cube root equation. This finding tends to confirm the suggestion that cube root relations may represent volume effects in terms of the linear activity of the central nervous system.

The equation for the convergence point of the two eyes is found to be similar to the Nernst-Hill equation for nervous excitation both in the form of the equation and in the character of the variables. It is possible that electrical effects based on the ionic activity of the central nervous system may be responsible for the perception of visual distance.

(Manuscript received April 26, 1938)

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BACKWARD CONDITIONING OF THE EYELID RESPONSE [1]

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The elaboration of conditioned responses in experimental situations where the onset of the unconditioned stimulus precedes that of the conditioned stimulus has been reported by Pavlov (7), Switzer (8), Wolfle (10), and Bernstein (1) among others. With the possible exception of Switzer's study, these investigations have indicated that backward conditioned responses were secured less frequently than the more conventional forward conditioned response. Switzer, however, reported elaboration of backward conditioned responses in 80 percent of his subjects. Bernstein found that in some instances the latency of these responses to the conditioned stimulus was of the same order as that of primary, or reflex, responses and he suggested that the high incidence of backward conditioning reported by Switzer was due to the failure of the latter to distinguish between long and short latency responses to the conditioned stimulus.

That the repeated presentation of a relatively intense stimulus may serve to lower the threshold of response to other stimuli has been reported by Wendt (9), Hilgard (3), Hull [2] and Bernstein (1). The latter two have reported data which appear to indicate that this 'sensitization' of the primary response does not depend upon the paired presentation of stimuli and hence such responses cannot be regarded as conditioned responses in the accepted usage of the term.

The aim of the present investigation was to study the modification and elaboration of responses to the conditioned stimulus resulting from a series of paired stimulations; the

[1] This investigation was carried on in the Psychological Laboratory of Yale University. The writer is indebted to Dr. Walter R. Miles for use of laboratory facilities.

[2] Personal communication.

onset of the unconditioned stimulus to precede that of the conditioned stimulus. The method employed to record reactions to stimulation was such as to permit the precise determination of the latency and amplitude of response.

Apparatus Procedure, and Subjects

Responses of the left upper eyelid to stmulation were recorded photographically by reflecting a beam of light onto the slit of a Dodge-Yale recording camera from a tiny mirror mounted upon the pivoted end of a light bamboo lever, the distal end of the lever being attached to the eyelid with a small piece of surgical tape. The unconditioned stimulus was a short puff of air directed against the cornea of the subject's left eye. The conditioned stimulus was a brief (ca 100 millisecond) flash of light which served to increase the illumination of the subject's fixation field. The administration of stimuli was controlled by a Dodge interval-timer and their onset recorded upon the photographic record. Accessory apparatus served to place time lines upon the record every twenty milliseconds. The latency of reactions could be accurately read to within 10 milliseconds. The subjects wore headphones the terminals of which were connected to the low voltage leads of a transformer thus providing a continuous auditory masking of extraneous sounds.

The procedure outlined below was followed with each subject and photographic records were made of all responses taking place within 2–3 seconds after the onset of the unconditioned stimulus.

5—presentations of light (the conditioned stimulus)
5—presentations of the air-puff (the unconditioned stimulus)
65—paired presentations of the unconditioned and conditioned stimulus
5—presentations of light.

Isolated presentations of the unconditioned stimulus were interspersed among the training trials as controls.

The interval between the onset of the unconditioned stimulus and that of the conditioned stimulus was approximately 470 milliseconds for nine of the subjects and approximately 980 milliseconds for the remaining eleven subjects. Hull has found that the 'sensitized' primary reaction weakens with the passage of time. Hence it might be argued that in the present experiment the 'sensitization' of the primary response to light would be greater the shorter the interval between the unconditioned and conditioned stimuli. Analysis of the records indicated no consistent or significant differences between the two groups of subjects employed in the present investigation. Therefore, no distinction between the two groups is made in the treatment of the data as reported in this paper.

The interval between successive paired presentations of stimuli was varied in a random fashion between ten and forty seconds. Isolated presentations of light, the conditioned stimulus, followed thirty seconds after the last paired stimulation.

The subjects employed in this investigation were twenty undergraduate students at Yale University. Only one of the subjects had had previous experience with conditioned eyelid experimentation. Two of the subjects had served in experiments concerned with the conditioned galvanic skin response. All subjects were kept in ignorance of the purpose of this investigation.

Results and Discussion

1. *Modifications in the Primary Response to the Conditioned Stimulus*

Figure 1 shows the average latency and amplitude of the primary responses to the conditioned atimulus before, during and after paired stimulation. The average latency of the primary response to the conditioned stimulus before paired stimulation was 99 milliseconds (SD = 8.95). After 65 paired stimulations the primary responses to light had an average latency of 101 milliseconds (SD = 7.05). The increase in

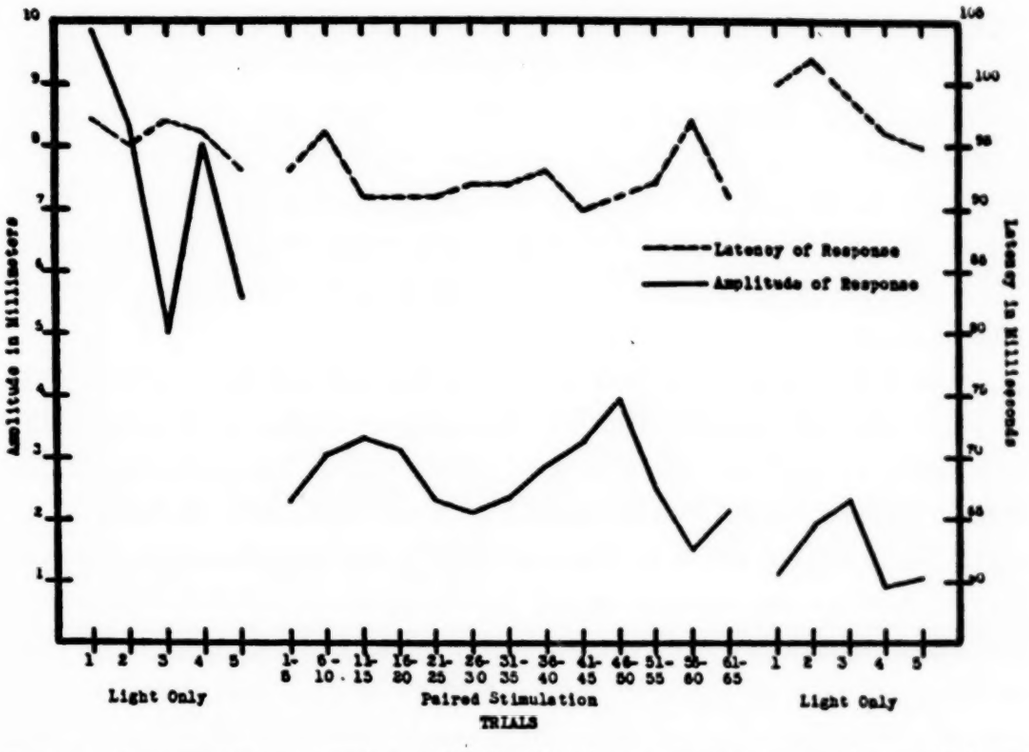

Fig. 1. Average latency and amplitude of primary responses to the conditioned stimulus (light) before, during and after paired stimulation.

latency is not significant (D/σ diff. = 1.26). The average latency of the primary response to light during the paired presentations of air and light was 92 milliseconds.

The average amplitude of the primary response to light before paired stimulation was 7.26 millimeters (SD = 8.60); absence of response being counted as zero. After paired stimulation the average amplitude of the primary response to the conditioned stimulus was 2.00 millimeters (SD = 1.96);

absence of response being counted as zero. Paired stimulation resulted in a decrement of the amplitude of the primary response to the conditioned stimulus which decrement is statistically significant (D/σ diff. = 5.6).

Figure 1 also shows that the primary response to light manifested an immediate decrement in amplitude with the beginning of paired stimulation. The work of Yerkes (**11**), Bowditch and Warren (**2**), and Hilgard (**4**), among others, indicates comparable results. Primary reactions to the second of two stimuli show facilitation when the stimuli occur in very close succession and inhibition when the interval between stimuli is lengthened within certain limits.

Within the limits of this investigation the data fail to substantiate Bernstein's hypothesis that the repeated presentation of a relatively intense stimulus facilitates the primary response to a second and succeeding stimulus. Thus what might be termed 'alpha' backward conditioning, after Hull's (**12**) suggestion, is not demonstrated by the data of this investigation.

Since the intervals between the unconditioned and conditioned stimuli employed by Bernstein (500 and 900 milliseconds) as well as the number of paired presentations of the unconditioned and conditioned stimuli (50) are closely comparable to those used in the present investigation the cause of the disparity in results must be sought in other differences between the two investigations. In this connection the nature of the unconditioned stimuli employed merits first consideration. Bernstein employed electric shock applied to a motor point to elicit a lid response. Repetition of Bernstein's procedure with the recording technique herein described appears desirable.

It should be noted that Hilgard and Biel (**5**) have recently reported failure to establish 'alpha' conditioning in an experimental situation closely resembling that of the present investigation save that the conditioned stimulus preceded the unconditioned and certain differences may have been obtained with respect to the intensity of stimuli.

2. *Long Latency Responses to Light During and After Paired Stimulation*

Table 1 indicates the frequency, latency and amplitude of what will be termed long latency responses to light after paired stimulation. The records of thirteen of the twenty subjects indicate one or more long latency (*i.e.*, over 120 milliseconds) responses to the conditioned stimulus. The records of six of these thirteen subjects indicate the presence

TABLE 1

SHOWING THE LATENCY AND AMPLITUDE OF SECONDARY RESPONSES TO LIGHT AFTER PAIRED STIMULATION

Subject No.	Trials				
	1	2	3	4	5
1*	0–0	0–0	570–3	305–9	530–13
2*	0–0	0–0	235–1	0–0	0–0
3	0–0	0–0	0–0	0–0	0–0
4	0–0	0–0	0–0	0–0	0–0
5	0–0	0–0	0–0	630–38.5	0–0
6	0–0	0–0	0–0	0–0	830–16
7*	580–32	460–30.5	0–0	535–25	0–0
8	0–0	0–0	970–14	575–18.5	540–12.5
9	0–0	0–0	0–0	920–3	0–0
10	0–0	0–0	0–0	0–0	0–0
11*	435–0.5	455–0.25	0–0	0–0	0–0
12*	0–0	0–0	360–4.5	390–1	
13	Record not readable				
14	0–0	420–0.5	0–0	0–0	0–0
15	0–0	0–0	0–0	0–0	0–0
16	190–0.5	0–0	0–0	0–0	0–0
17	0–0	0–0	0–0	0–0	0–0
18	0–0	0–0	0–0	0–0	0–0
19*	0–0	0–0	390–5.5	0–0	0–0
20	0–0	0–0	0–0	900–30.5	0–0

0–0 indicates no response to stimulation. First figure indicates latency in milliseconds; second indicates amplitude in millimeters.

* Record indicates long latency response to light present before paired stimulation.

of one or more long latency responses to light before paired stimulation. Thus in only seven subjects is backward conditioning in the usual sense of the term apparently indicated. It would be extremely incautious, however, to label these responses as such without further consideration. In the absence of a 'ready signal' before stimulation these apparent

responses to light may be nothing more than spontaneous reactions, especially since repeated application of the unconditioned stimulus might be expected to have increased spontaneous activity.

The work of Hilgard (**3**), and Hilgard and Marquis (**6**) has shown that the development of forward conditioned responses during paired stimulation is characterized by (1) the elaboration of responses of an unique latency which (2) show recruitment and (3) increased frequency of incidence with training.

If the long latency responses to light after paired stimulation are conditioned responses rather than spontaneous reactions their development should be apparent in the records made during paired presentation of stimuli. Examining the records of these seven subjects for responses to light having an unique latency showed that responses to light having a latency between 120 and 1000 milliseconds show the distribution illustrated in (*A*) of Table 2.[3] This appears to be a bimodal distribution with modes at 175 and 425 milliseconds. Reference to Table 1 will show that only three of the possible conditioned responses after paired stimulation fall near one or the other of these modal points. This fact forces us to the tentative conclusion that the responses listed in Table 1 represent spontaneous rather than conditioned reactions.

Table 2 also indicates the average modification in amplitude of responses having a latency between 120 and 550 milliseconds in addition to the frequency of incidence of such responses during paired stimulation. It will be noted that these values indicate neither recruitment in amplitude nor increased frequency of incidence.

These facts serve to confirm the tentative hypothesis mentioned above; namely, that the responses shown in Table 1 were spontaneous rather than conditioned reactions. The absence of recruitment in amplitude and increased frequency of incidence also holds when all long latency responses for all subjects are treated in this manner.

[3] The lower limit was selected because it was approximately 3 SD above the mean of the primary reactions to light before paired stimulation; the selection of the upper limit was dictated by practical considerations.

TABLE 2

Showing (*A*) the distribution of latencies of secondary responses to light during paired stimulation; (*B*) the average amplitude of secondary responses to light during paired stimulation having latencies between 120 and 550 milliseconds; (*C*) the frequency of secondary responses to light during paired stimulation having latencies between 120 and 550 milliseconds. Table based on data obtained with subjects showing long latency responses to light after, but not before, paired stimulation.

(*A*)		(*B*)		(*C*)	
Milliseconds	Frequency	Trials	Average Amplitude in Millimeters	Trials	Frequency
999–950	1	1– 5	6.6	1– 5	9
949–900	3	6–10	5.7	6–10	7
899–850	5	11–15	2.7	11–15	6
849–800	6	16–20	4.5	16–20	8
799–750	3	21–25	4.8	21–25	8
749–700	5	26–30	7.8	26–30	8
699–650	4	31–35	4.9	31–35	7
649–600	6	36–40	4.7	36–40	7
599–550	1	41–45	8.0	41–45	8
549–500	11	46–50	3.9	46–50	8
499–450	10	51–55	2.9	51–55	3
449–400	16	56–60	7.0	56–60	8
399–350	7	61–65	5.6	61–65	6
349–300	8				
299–250	13				
249–200	15				
199–150	17				
149–100	5				

SUMMARY

Backward conditioned eyelid responses were not secured after sixty-five paired presentations of air (the unconditioned stimulus) and light (the conditioned stimulus). The paired stimulation did not result in the sensitization of the primary response to the conditioned stimulus.

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STUDIES IN THERMAL SENSITIVITY. 7. FURTHER SYNTHETIC EVIDENCE AGAINST THE ALRUTZ THEORY

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Two previous studies in this series have presented the results of synthetic experiments which indicate that the Alrutz theory of 'heat' (simultaneous warm plus paradoxical cold) does not apply to the common experience 'hot.' Warm + cold grill stimulation of naive subjects does not result in consistent reports of 'hot,' which actually occur *less* frequently with this compound than with the corresponding simple warm stimulation alone.[1] 'Hot' and 'burning hot' can be synthesized from simple warm + electric shock in a fair number of instances, but the addition of cold to the complex sharply reduces the positive effects.[2]

Following an informal presentation of these results, objections were raised to the *technique* on two grounds: (1) Flat bar grills had been used in these studies, whereas the original synthetic experiments with trained observers had been performed with grills made of round tubing.[3] (2) Subjects had not been instructed to press down hard on the grill, and strong pressure was said to aid the synthesis. The best answer to these objections is the outcome of direct experimental tests.

[1] Jenkins, W. L., Studies in thermal sensitivity: 5. The reactions of untrained subjects to simultaneous warm+cold stimulation, *J. Exper. Psychol.*, 1938, **22**, 451–461.

[2] Jenkins, W. L., Studies in thermal sensitivity: 6. The reactions of untrained subjects to simultaneous warm + cold + electric shock, *J. Exper. Psychol.*, 1938, **22**. 564–572.

[3] For economy of space, these will hereafter be referred to as the 'Dallenbach experiments,' since the best of them were carried out in the Cornell laboratory under the guidance of Dr. K. M. Dallenbach. For a bibliography and brief review, see: Jenkins, W. L., Studies in thermal sensitivity: No. 5. The reactions of untrained subjects to simultaneous warm + cold stimulation, *J. Exper. Psychol.*, 1938, **22**, 451–453.

Apparatus and Procedure

Two additional grills were constructed for this work: No. 5, of 3 mm diam. copper tubing with 1½ mm air gap between tubes; No. 6 of 4½ mm diam. and 2 mm air gap. In both grills, the total stimulating surface was approximately 8 × 7 cm, and the cold tubes could be raised and lowered, permitting a direct comparison between simple warm stimulation and the same warm compounded with cold. Grill No. 1, with 3 mm flat bars, was used as a control.

One pair of temperatures (40° C. and 20° C.) was employed throughout. Each subject was tested 10 times with each of the three grills, with the simple stimulation preceding the compound; and also 4 times with Grill No. 1 when the compound stimulation was given first. Half of the stimulations were made with light pressure and half with heavy pressure, in random order. For light pressure, the subject merely allowed the volar surface of his left forearm to rest upon the grill. For heavy pressure, he was instructed to bear down as hard as he could. All grills were concealed by a hood. Reports were made in terms of: cold, neutral, warm, hot or burning hot.

Results

Table 1 shows that the results were as flatly negative as

TABLE 1

Percentages—Reports of 'Hot'

Grill No.	Pressure	Simple 40° Alone	Compound—40° and 20°	
			Total	Clear
1	Light	15.5	0.0	0.0
1	Heavy	16.2	0.7	0.9
1*	Light	6.9	10.3	12.3
1*	Heavy	5.2	5.2	5.5
5	Light	10.7	2.1	2.4
5	Heavy	12.8	2.1	0.8
6	Light	9.3	3.1	3.4
6	Heavy	12.4	2.5	2.8

* Compound stimulation given first.

those obtained in the earlier study, neither tubular grills nor heavy pressure having any significant effect. Heavy pressure increased the reports of 'hot' from simple stimulation, but actually reduced the small number obtained with the compound. When the compound stimulation was given first, the 'clear hot' reports with light pressure rose to 12.3 percent. But even this figure must be viewed with suspicion, because the simple stimulation itself gave 15.5 percent.

Table 2 confirms the analysis. 19 of the subjects never gave a single clear report of 'hot' with compound stimulation.

TABLE 2

CLEAR REPORTS OF 'HOT' BY SUBJECTS

Never	19
0–9 percent	8*
10–24 percent	1*
25–29 percent	1
Over 30 percent	0
Total	29

* In 7 of these 9 cases, 'hot' was reported a greater number of times with simple stimulation.

Nine others did report it sporadically, but 7 of these called the simple stimulation 'hot' more frequently. There is just one apparently genuine case of synthesis . . . the individual who reported 'hot' with 26 percent of the compound stimulations and never called the simple stimulation 'hot.' A few similar instances were found in the earlier study.

DISCUSSION

These experimental tests seem to dispose of the objections on the grounds of technique. Considering that negative results were also secured by Dallenbach in the single instance when a naive individual was tested,[4] it appears unlikely that further variations of warm + cold synthesis would yield any significant number of reports of 'hot' from untrained subjects.

Another objection which has been raised to these negative results is that they are *meaningless*. Naive individuals, it is argued, do not report 'heat' in response to simultaneous warm + cold stimulation, because they have not been properly trained to recognize 'heat' when they encounter it. Hence, their testimony on the subject is worthless.

This objection is cogent only if we assume that there is one universal experience from high temperature stimulation. But why should we make this assumption? On both physiological and psychological grounds, there is reason to expect a variety of experiences. Physiologically, the warm, cold and pain receptors might respond differently in various individ-

[4] Ferrall, S. C. and Dallenbach, K. M., The analysis and synthesis of burning heat, *Amer. J. Psychol.*, 1930, **42**, 76.

uals. Psychologically, in the perception called 'heat' or 'hot,' the roles played by warm, cold and pain excitation might vary in different people—even if the physiological substrate was identical in all cases.

When we approach the literature from this viewpoint, we can distinguish at least *five* variant definitions of 'heat' or 'hot.' These may be briefly summarized as follows:

1. For Goldscheider, 'heat' was merely a higher degree of 'warm,' but because of the long-lasting after-sensations, he held that stimulation of the *Gefühlsnerven* was also involved. Cold sensations might or might not be present, but these were adventitious and could be analyzed out of the mixture.[5]

2. For Kiesow, the participation of the pain receptors was an essential part, of what he called 'heat.' where the experience was not actually painful, it was always *schmerzbetont.* Cold receptor excitation was not essential, because 'heat' could be experienced in regions where no cold receptors could be demonstrated.[6]

3. For Alrutz, 'heat' was a unique and unanalyzable experience, easily distinguishable from 'warm.' Its critical feature, he held, was the participation of cold receptor excitation, because 'heat' could not be experienced where cold receptors were absent.[7]

4. For Hacker and also for Thunberg, although cold receptor excitation was involved, 'heat' was *not* a unique and unanalyzable experience. Rather, it was a *Mischempfindung* in which the warm and cold elements could be distinguished with practice.[8]

5. In addition to these four different 'heat' experiences

[5] Goldscheider, A., Beiträge zur Lehre von der Hautsensibilität, *Zsch. f. klin. Med.*, 1912, **75**, 9–10.

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[8] Hacker, F., Beobachtungen an einer Hautstelle mit dissozierter Empfindungslähmung, *Zsch. f. Biol.*, 1913, **61**, 242. Thunberg, T., Untersuchungen über die relative Tiefenlage der kälte-, wärme- und schmerzperzipierenden Nervenden in der Haut und über das Verhältnis der Kaltenervenden gegenüber Wärmereizen, *Skand. Arch. f. Physiol.*, 1901, **11**, 415.

described by trained research workers, it seems reasonable to grant the existence of a fifth, which we have termed the common experience 'hot.' For most naive individuals, 'hot' is merely a *pseudo-discrete* step in an actual continuum. 'Barely warm' changes to 'warm' and to 'very warm' and to 'hot,' without any sharp dividing lines. The numbers 1, 2, 3, 4, etc. would serve equally well. As far as the present evidence shows, cold excitation plays no essential role.

From this varied array of experiences, we may choose that of Alrutz and arbitrarily label it 'true heat.' Then, quite obviously, only those who have this particular experience are qualified to take part in experiments concerning its nature. This eliminates the evidence of Goldscheider, Kiesow, Hacker and Thunberg, because these men did not experience 'true heat.' The same objection applies to studies with untrained subjects.

Fortunately, the trained observers in the Dallenbach experiments did experience 'true heat'—unique and unanalyzable, qualitatively different from 'warm'—and this could be synthesized consistently from simultaneous warm + cold stimulation. Unfortunately, such synthetic evidence does not prove that cold excitation is a necessary part of the natural experience. Somewhat disturbing are the casually-reported findings of Knight, who synthesized 'burning heat,' 'burn' and 'hotness' from punctiform warm + pain in the same laboratory. Is it possible that the 'synthetic heat' from warm + cold is merely an illusion? Final judgment must await the results of analytic tests to show whether 'true heat' can be experienced where no cold sensitivity exists.

Meanwhile, there remains the problem of what to do about the common experience 'hot.' Although it is clearly not 'true heat,' it seems to be the experience of the bulk of ordinary individuals. Hence, its constitution may be of interest to general psychology. Does the Alrutz theory of warm plus paradoxical cold excitation apply? The negative results with attempted warm + cold synthesis provide a strong presumption (but not proof) that it does not. The final answer must come from critical analytic tests.

SUMMARY

Additional experiments with simultaneous warm + cold stimulation, employing tubular grills with light and heavy pressure, confirm the negative results previously secured with flat bar grills. Untrained subjects do not report 'hot' in response to warm + cold stimulation. To the objection that these results are meaningless because naive individuals have not been properly trained to recognize 'heat' . . . the following answer is made: Five clearly-different experiences called 'heat' or 'hot' have been described in the literature, four of them by trained investigators. We may arbitrarily select the experience of Alrutz and label this 'true heat.' But the common experience 'hot' is also of interest to general psychology. According to the synthetic findings, cold excitation is probably not an essential element and the Alrutz theory does not apply. Final judgment must wait upon more critical analytic evidence.

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STUDIES IN THERMAL SENSITIVITY. 8. ANALYTIC EVIDENCE AGAINST THE ALRUTZ THEORY

BY WILLIAM LEROY JENKINS

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Three preceding articles in this series have presented the results of synthetic experiments which indicate that the Alrutz theory of 'heat' (simultaneous warm plus paradoxical cold) does not apply to the common experience 'hot.' With untrained subjects, 'hot' cannot be synthesized from simultaneous warm + cold stimulation with flat bar grills,[1] nor with tubular grills regardless of whether light contact or heavy pressure is employed.[2] 'Hot' and 'burning hot' can be derived in a fair percentage of cases from simple warm + electric shock, but the addition of cold tends to destroy the effect.[3]

Neither these negative results with untrained subjects nor the positive results obtained by Dallenbach and others provide critical evidence concerning the constitution of 'hot' or 'heat.' The negative results may be an artifact of faulty technique. The positive results may be illusory. Whether cold excitation is a necessary part of the *natural* experience can be checked only by direct analytical attack.

Analytically, the critical question is: "Can 'hot' (or 'heat') be experienced in skin areas where cold sensitivity is either absent or not functioning?" If 'hot' (or 'heat') can be consistently experienced under these conditions, then cold excitation is clearly not a *necessary* part of the experience and the Alrutz theory does not hold.

[1] Jenkins, W. L., Studies in thermal sensitivity: 5. The reactions of untrained subjects to simultaneous warm + cold stimulation, *J. Exper. Psychol.*, 1938, **22**, 451–461.

[2] Jenkins, W. L., Studies in thermal sensitivity: 7. Further synthetic evidence against the Alrutz theory, *J. Exper. Psychol.*, 1938, **22**, 411–416.

[3] Jenkins, W. L., Studies in thermal sensitivity: 6. The reactions of untrained subjects to simultaneous warm + cold + electric shock, *J. Exper. Psychol.*, 1938, **22**, 564–572.

No accounts of systematic analytic studies of this nature are found in the literature. Alrutz states categorically that 'heat' cannot be experienced in regions where cold sensitivity is absent, with which Hacker and Thunberg agree, although their definition of 'heat' is not the same as his. Goldscheider and Kiesow are equally positive that 'heat,' as they use the term, can be experienced in regions where no cold sensitivity can be demonstrated. In no case, however, is detailed evidence reported. Nor were such systematic tests carried out with the introspectively-trained observers of the Dallenbach experiments. The work to be reported here is an attempt to answer the critical question only for the common experience 'hot.'

Procedures

1. *With Cold-adapted Areas.*—Subjects were divided into groups of three, each individual acting in turn as experimenter, subject and recorder. Three circles (13 mm diam.) were stamped on the volar surface of the left forearm. A stimulator 13 mm in diam., maintained at 18–20° C. by circulating water, was held against the arm within a marked circle for three minutes (unless complete adaptation occurred sooner). A stimulator 2 mm diam. at 50° C. was then applied at random inside and outside of the circles for a total of 10 stimulations each, the subject having his eyes closed. This was repeated in rotation, using a different circle each time, until each subject had received 60 stimulations (30 inside and 30 outside of the circles). The subjects reported: cold, neutral, warm, hot or burning hot.

2. *With Cold-mapped Areas.*—This experiment was also carried out in groups of three. The usual precautions in mapping were observed, and the members of each group rotated positions every few minutes to minimize the effects of adaptation. In an area 2 × 2 cm, the parts sensitive to cold (2 mm diam. stimulator at 18–20° C.) were marked directly on the skin. The stimulator was then brought to 50 C. and each mapped area was stimulated in random order for a total of 10 stimulations within the cold-sensitive parts and 10 in the non-cold-sensitive. This was repeated in rotation for a total of 60 stimulations for each individual (30 in cold, and 30 in non-cold). The subjects reported: cold, neutral, warm, hot and burning hot.

3. *With Seriatim Stimulation.*—In this procedure, the author acted as experimenter and the subjects were tested in groups of 3 to 8. A block of 25 spots (each about 3 mm square and about 3 mm apart) was stamped on the volar surface of the left forearm. The 25 spots on each subject were stimulated in random order with a 3 mm diam. stimulator, the members of the group being handled in rotation to minimize the effects of adaptation. Low temperature stimulation was alternated with high temperature stimulation, until 6 low (3 at 20° C. and 3 at 10° C.) and 5 high (the same throughout, but varying between 40° C. and 50° C. with different groups) had been completed upon each subject. In one set (49–50° C.), only 5 cold stimulations, all at 20° C., were used.

Results

1. In the first procedure, the idea was to render the cold receptors inoperative through long-time adaptation. 56 subjects were stimulated with 50° C. 1,614 times within the cold-adapted circles and an equal number of times on the normal skin outside. 'Hot' or 'burning hot' was reported 355 times from within the circles and 462 times from outside. Three objections might be raised to these results: (1) There is no direct evidence that cold-adaptation was complete as far as paradoxical stimulation is concerned. (2) Just such results might be expected if the subjects were reporting 'warm' and 'hot' at random without regard to the actual experience. (3) On the other hand, the positive results might be reduced an undetermined amount by the effect of the prolonged pre-cooling on the warm receptors. In fact, this is indicated by the reduced *total* number of warm and hot reports . . . 1,049 from outside and 930 from inside the circles.

2. In the second procedure, the idea was to compare the results from cold and non-cold-sensitive areas. 73 subjects were stimulated with 50° C. 2,190 times in non-cold-sensitive parts and an equal number in the cold-sensitive parts. 'Hot' or 'burning hot' was reported 560 times from the former and 618 times from the latter. Three objections might be raised to these findings: (1) With untrained subjects, the cold-mapping was undoubtedly inaccurate. (2) Figures like these could result from purely random reporting. (3) There might be cold receptors in the area which responded to paradoxical stimulation, but not to normal stimulation at 18–20° C.

3. The third procedure was designed to meet these objections. The seriatim scheme of stimulation provides evidence of the consistency of reports from any given spot, eliminating the possibility that the results could be due to random reporting. Furthermore, if there are actually some cold receptors which respond better to paradoxical than to normal stimulation, a considerable number of reports of paradoxical cold should be obtained from spots that were neutral to normal cold stimulation.

Table 1 shows the gross results for 64 subjects according to three different criteria of consistency. Spots that were consistently 'hot' (to high temperature stimulation) were relatively as frequently 'neutral' (to low temperature stimulation) as they were 'cold' (to low temperature stimulation). With perfect consistency as the criterion, the percentages are almost identical. With less exacting criteria, the equality relationship is unchanged.

Concerning the possibility of spots which respond to paradoxical but not to normal low temperature stimulation, the record is equally clear. Out of 10,625 stimulations at

TABLE 1

SPOTS CONSISTENTLY CALLED 'HOT' AS COUPLED WITH 'COLD' AND WITH 'NEUTRAL'

Criterion of Consistency		'HOT' Coupled with 'NEUTRAL'	'HOT' Coupled with 'COLD'
For 'Hot'	For 'Cold' or 'Neutral'		
5/5	6/6	44/140 *31.4%*	145/482 *30.2%*
4/5	5/6	121/288 *42.0%*	340/787 *43.3%*
3/5	4/6	203/438 *46.3%*	499/1008 *49.6%*

Note: Percentages are computed in terms of total spots in each group giving positive responses to high temperature stimulation. For example: 140 spots were positive 5 times out of 5 high temperature stimulations and 'neutral' 6 times out of 6 low temperature stimulations. Of these 140 spots, 44 were 'hot' 5 times out of 5 high temperature stimulations—31.4 percent. The other figures in the table were similarly derived.

temperatures between 40° C. and 50° C., paradoxical cold was reported exactly 16 times in all—in every case from spots which were consistently 'cold' to normal stimulation. There is not even a single instance of a report of paradoxical cold from a spot which was consistently neutral to normal low temperature stimulation.[4]

[4] Reports of paradoxical warm occurred 85 times out of 4,650 stimulations at 20° C., and 25 times out of an equal number at 10° C. In spite of the comparative rarity of both paradoxical phenomena, there is some evidence of their genuineness. On two occasions, paradoxical cold was reported 3 times from the same spot. In two cases, paradoxical warm was reported 4 times from the same spot; and in 8 cases, 3 times from the same spot.

Discussion

Unless serious exception can be taken to all three of the procedures reported, it seems quite clear that cold excitation is not necessary for the common experience 'hot.' Untrained subjects report 'hot' with approximately equal frequency from 'cold' spots and from 'neutral' spots. Thus the provisional conclusion from the negative results with warm + cold grill stimulation is confirmed. *The Alrutz theory does not apply to the common experience 'hot.'*

It will be readily admitted that these subjects were not experiencing 'heat' . . . in the Alrutz sense of the term. In fact, that is exactly our point. The Alrutz 'heat' is not the same as the common 'hot.' As Alrutz described it, 'heat' is unique—qualitatively different from 'warm.' As the untrained subject describes it, 'hot' is merely a higher degree of 'warm' and not qualitatively different. This latter appears to be the experience of most naive individuals.

Further research may reveal that some individuals *spontaneously* have the experience of Alrutz from high temperature stimulation. Again, it may be possible to *teach* individuals to have such a perception, although they did not have it originally. In either event, the acid test is not the effect of warm + cold synthesis, but rather some analytical technique, preferably seriatim stimulation of a large number of marked spots. Then, if cold excitation is essential for the experience, the subject should never report 'heat' from a spot that is consistently neutral to low temperature stimulation.

It seems fair to challenge the supporters of the Alrutz theory to perform such experiments with introspectively-trained observers. When a trained observer states that 'heat' is unique and qualitatively different from warm, that is a subjective judgment. When a trained observer says that his experience from simultaneous warm + cold grill stimulation is identical with 'heat,' that is another subjective judgment.

But when a trained observer holds that 'heat' is *dependent* upon paradoxical cold excitation, that is clearly a matter for objective check. He can submit himself to the crucial test of seriatim stimulation. If he reports 'heat' from any spots

that are consistently neutral to cold stimulation, then cold is not a necessary part of his experience—and the Alrutz theory does not apply to it. If he reports 'heat' only from spots which are consistently cold to low temperature stimulation this does not necessarily prove the Alrutz theory. It remains to be shown that paradoxical stimulation of cold receptors does actually occur every time 'heat' is experienced. But this is merely tilting at windmills. Let us first see whether trained observers, working without knowledge of which spots are cold and which are neutral, will report 'heat' only from 'cold' spots and never from 'neutral' spots. That is the next step to be taken.

Meanwhile, it is safe to say that the Alrutz theory should not be stated as a generalization. It clearly does not hold for the common experience 'hot.' From a critical standpoint, there is absolutely no direct, objective evidence that it has ever applied to anyone but Alrutz himself.

Summary

Three procedures were used to check whether cold excitation is necessary for the common experience 'hot': (1) reports of 'hot' from cold-adapted and normal skin; (2) reports of 'hot' from mapped cold and non-cold-sensitive areas; (3) seriatim stimulation of 25 marked spots, alternately with high and low temperatures. Results from all three procedures were in agreement: 'hot' is reported whether 'cold' is present or not. Clearly, cold excitation is not a *necessary* part of the common experience 'hot' and the Alrutz theory does not apply to it. Supporters of the Alrutz theory are challenged to apply similar tests to trained observers. There is no objective evidence which shows that the Alrutz theory applies even to what trained observers call 'heat.'

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COMPARISON OF THE INFLUENCES OF MONETARY REWARD AND ELECTRIC SHOCKS ON LEARNING IN EYE-HAND COÖRDINATION

BY ROLAND C. TRAVIS

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Introduction

This study is concerned with the relative influences of electric shock and monetary reward as motivating factors in learning a motor skill in eye-hand coördination. The effects of electric shock on learning in eye-hand coördination were reported in a previous article.[1] Three different experimental situations were employed in the former study with three different groups of college men. In the first situation no shocks were administered during any of the trials and the normal learning curve was obtained. In the second situation shocks were given during each trial whenever the stylus lost contact with the target. Comparisons between all three situations and between the gains of the shock-trials and the non-shock-trials in the third situation were made. Shocks received during each trial were slightly more disrupting than shocks received during alternating trials as exhibited in the general learning curve. The gains for the shock-trials were slightly higher than those for the non-shock-trials. Differences between the subjects who stated that they were annoyed and those who were not annoyed by the shocks were too small to be statistically significant. Any of these differences between the various situations were not very large, the critical ratios ranged from 0.2 to 1.8.

Procedure

The experimental setup used in the previous study was also used in the present study. The learning task was to hold a flexible stylus on an insulated moving target

[1] Travis, R. C. and Anderson, H. C., The effect of electric shock on learning in eye-hand coördination, *J. Exper. Psychol.* 1938, **23**, 101–107.

which oscillated in front of the standing subject at the rate of one complete oscillation in one second. An electric counter recorded the score as long as the subject held the stylus on the target. Nine consecutive trials one minute in length with rest periods of three minutes were given each subject. Each subject was instructed that at the end of some trials he would receive a monetary reward of ten cents if he made a score of a certain magnitude. He was also instructed just before each trial whether or not he would be rewarded at the end of that trial. He was requested to do his best during all the trials and not to inquire about his score until the end of the experiment. Trials number 2, 4, 6 and 8 were the rewarded trials, no rewards were given for trials 1, 3, 5, 7 and 9. To control the effect on future trials of the monetary rewards, the reward of ten cents was given at the end of trials 2, 6 and 8, to all subjects regardless of the magnitude of the score. The subject was led to believe, however, that he must obtain a certain magnitude in his score in order to obtain the reward. This experimental deception seemed essential in order to produce comparable mental sets in the subjects. Each subject received 30 cents for his efforts regardless of his score.

The procedure for the subjects receiving shocks during trials 2, 4, 6 and 8 was given in detail in the previous study and will not be repeated here.

Results

Comparisons will be made between the gains of the group of subjects who received shocks during alternating trials and the gains of the group of subjects who expected to receive monetary rewards at the end of alternating trials and who actually received rewards at the end of trials 2, 6 and 8. Table 1 gives the average raw scores for the nine trials for

TABLE 1

Total Average Scores per Trial for the Shocked Group and the Rewarded Group

Trials	Shock Alternating Trials 27 Subjects—Average Score		Reward Alternating Trials 9 Subjects—Average Score	
	Score	Gain	Score	Gain
1	354		370	
2	445	91	400	30
3	491	46	470	70
4	539	48	500	30
5	563	24	510	10
6	576	13	580	70
7	607	31	560	−20
8	610	3	600	40
9	611	1	600	0
Total gain		257		230
Total percentage gain		42		38
Ave. percent gain per trial		5.25		4.75

the group which received electric shocks and the group which received monetary rewards.

There is a small difference of 4 percent in favor of the subjects who received shocks over those who received rewards in the total gain from the first trial to the last trial with a critical ratio of less than 1.00. Figure 1 shows clearly the

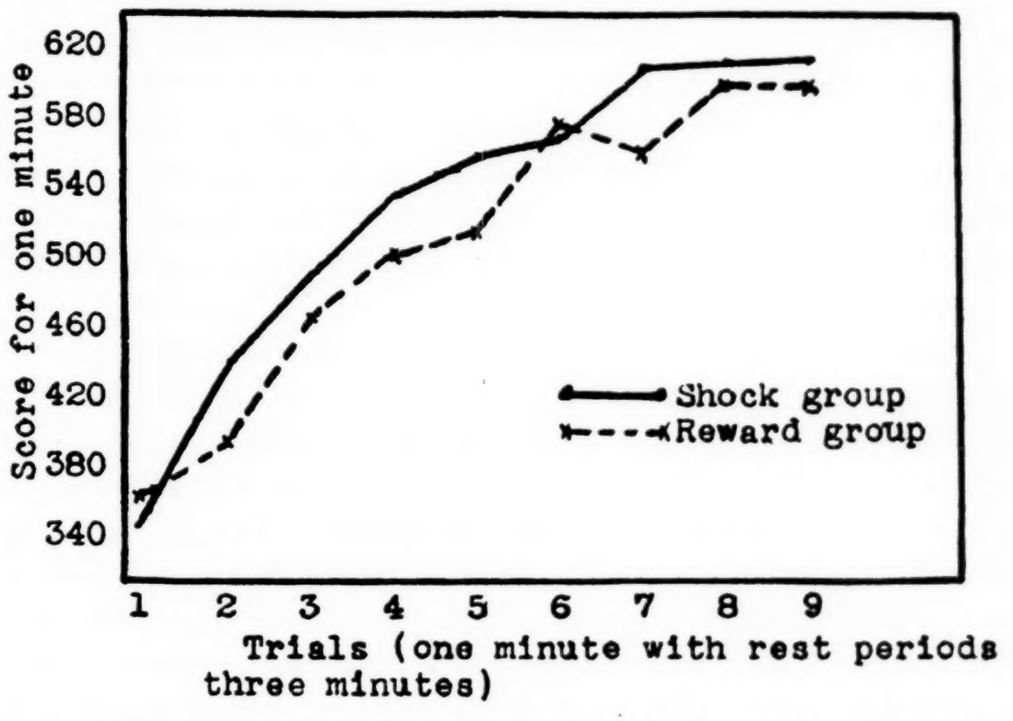

FIG. 1. Learning curves for subjects who received shocks and those who received rewards during alternating trials in eye-hand coördination.

differences from trial to trial between the two learning situations. On the first trial the rewarded group was 4 percent higher than the shocked group. The rewarded group again surpassed the shocked group on trial six by a very small percentage. For all the other trials, however, the shocked group was considerably better than the rewarded group. In comparing the gains for individual trials in each situation we found that the gains for the shock-trials (2, 4, 6 and 8) averaged about 4 percent higher than the gains for the non-shock-trials with a critical ratio of 1.8 using the standard error formula. The gains for the rewarded-trials (2, 4, 6 and 8) averaged about 4 percent higher than the gains for the non-rewarded-trials and because of the large variability in the

distribution, the critical ratio was only 0.9. These differences become less significant when we consider the differences between the gains made in trials 2, 4, 6 and 8 and the gains in trials 3, 5, 7 and 9 in the control group situation in the previous study wherein the normal learning curve was obtained without any experimental disrupting or reënforcing stimuli. In that study we found that the gains for trials 2, 4, 6 and 8 were two percent higher than the gains for trials 3, 5, 7 and 9 with a critical ratio of 0.8. In other words the *position* of the trials during which rewards or shocks were given in the total series seemed to be a factor of some importance. Therefore, the gains noted for the shock-trials and the reward-trials over the normal-trials indeed have very doubtful statistical significance.

Returning again to Fig. 1, it will be noted that the gain for trial three (a non-rewarded trial) for the rewarded group is greater than that for either trial 2 or trial 4 which were rewarded. The gain for trial 6 (a rewarded trial) was as great as any made by the rewarded group. This outstanding gain in later trials in this type of learning is rather unusual and may be explained on the basis of what has gone before. The subjects were all rewarded after trial 2 regardless of the size of the score. Having received the reward the impression that the reward was easily obtained may have been obtained. Then expecting another reward at the end of trial 4 and not getting it regardless of the size of the score seemed to have the opposite effect, namely, that the score was not high enough and extra effort must be exerted in order to earn the reward. At first the taste of victory then the experience of failure seemed to operate as an added incentive in producing the outstanding gain for trial 6 with a subsequent loss on the next trial and then regaining more than the loss on the eighth trial.

Summary of Conclusions

Several tentative conclusions from this study may be given as follows bearing in mind the statistical insignificance of the differences.

1. The method of comparing the gains made during the

shock-trials of reward-trials with the gains made during the normal-trials for the same subject within one experimental sitting is a feasible method of studying the effects of disrupting or reënforcing stimuli on motor learning.

2. The gains for the shock-trials were slightly higher (4 percent) than those for the non-shock-trials for 27 college men.

3. The gains for the reward-trials were slightly higher (4 percent) than those for the non-rewarded-trials for 9 college men.

4. The gains for the control group for trials 2, 4, 6 and 8 which were comparable to the serial position of the shock-trials and the reward-trials were slightly higher (2 percent) than the gains for trials 3, 5, 7 and 9. In other words, the serial position of the shock-trials and reward-trials was a factor of some importance.

5. Comparisons between the total average gains of the shock-group and the reward-group showed a difference of 4 percent in favor of the shock-group, with a critical ratio of less than 1.00 (standard error formula).

Within the experimental limits of these studies it is evident that electric shocks and monetary rewards of the character that were employed have some effect on learning in eye-hand coördination either as a slight disruption or a slight reënforcement. The general form of the learning curve, however, seems to remain about the same regardless of the shocks or the rewards.

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THE INFLUENCE OF THE DIFFICULTY OF ACTIVITY ON THE ESTIMATION OF TIME. II*

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After it was found in some investigations on time judgments, in which the activities had been listening to metronome beats under varying conditions, the difficulty of the activity appeared to be an important variable, it was decided to employ other activities varying in difficulty.

The activities employed in this experiment were judging weights under contrasting conditions of making difficult and easy discriminations.

Procedure and Materials

The subject sat on one side of a table, the experimenter across from him. There was a low screen between the experimenter and the subject that prevented the subject's observing the weights used and the records made by the experimenter.

Cylindrical bakelite weights, two and one-half inches in diameter, one and one-sixteenth inches high, weighing from eighty to one hundred twelve grams, were employed. Two series were arranged in advance, one to provide relatively large differences in weight to make judgments relatively easy; one to provide relatively slight differences to make judgments relatively difficult. The difficult series was: 101, 108, 101, 96, 92, 98, 104, 108, 101; the easy: 112, 101, 92, 80, 88, 96, 108, 92, 84, 96, 112. There were such wide variations in the individuals' abilities to make the discriminations, however, that these series were used only as guides.

After a preliminary period of practice two series were determined for each individual to provide contrasting activities, difficult and easy sensory discriminations.

The experimenter gave the subjects the following instructions:

"During the next periods, I want you to discriminate between some weights. I shall pass out weights to you, one after another. You are to lift the weights between your thumb and fingers, so (demonstrating), and say merely, 'equal,' 'heavier' or 'lighter,' according to your judgment, comparing each weight with the one that immediately precedes it. There are a few equals, not many. The weights are very close together, however, and you are bound to make some mistakes, but do not let that bother you. I shall not tell you whether you are right or wrong. Do the best you can. Try a few for practice."

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* This investigation was conducted at Duke University during 1935 and 1936 as a partial basis for a doctor's dissertation.

Several weights were passed out at this point to enable the subject to understand the procedure and assist the experimenter in deciding the relative differences the subject could discriminate easily and with difficulty. The subjects were told that they would be asked for time-estimates of the several periods spent at the activity.

After a brief practice period, the experimenter said, "Do you have any questions?" Usually there was none, and then the experimenter said, "Very well, we shall begin. This is the first weight of this series."

The weights were passed out, then, one after another, and taken back as soon as they were lifted. The subjects made, on an average, twelve judgments per minute. Some made more; some, less.

Subjects and Order of Trials

After thirty-three subjects had the trials, it was discovered that no matter which of the contrasting activities came first, there was a tendency for the very first period to be estimated longer than the second. On account of this variable, it was determined to get more subjects who would have the activities in reverse order from that had by the first group of subjects.

Thirty-three subjects [1] had the following sequence of periods. This is called Sequence Number One.

	Order of Trials					
	First	Second	Third	Fourth	Fifth	Sixth
Duration in seconds	135	135	180	180	120	120
Characterization of activity	Easy	Difficult	Difficult	Easy	Easy	Difficult

Nine subjects [2] had the following series of periods. This is called Sequence Number Two.[3]

	Order of Trials					
	First	Second	Third	Fourth	Fifth	Sixth
Duration in seconds	135	135	180	180	120	120
Characterization of activity	Difficult	Easy	Easy	Difficult	Difficult	Easy

Twenty-five subjects had the following series of periods. This is called Sequence Number Two, also. These subjects

[1] These were the same subjects who served in other laboratory experiments, undergraduate students.

[2] These were graduate students picked at random.

[3] Both the following two series are called Number Two because the order of Difficult-Easy sequence is the same.

	Order of Trials					
	First	Second	Third	Fourth	Fifth	Sixth
Duration in seconds.....	135	135	75	75	120	120
Characterization of activity.....	Difficult	Easy	Easy	Difficult	Difficult	Easy

were undergraduate women who served as subjects for the repetition of three other experiments. Due to lack of time it was necessary to substitute two short periods of 75 seconds in these trials in place of the two periods of 180 seconds, and even then only fifteen of these subjects made three pairs of judgments.

Thus, thirty-three subjects had the trials in Sequence Number One, Easy-Difficult-Difficult-Easy-Easy-Difficult; thirty-four had Sequence Number Two, Difficult-Easy-Easy-Difficult-Difficult-Easy.[4]

Method of Obtaining Estimates

As soon as the subject made his last discrimination in the first period, the experimenter said, "Now, what is your estimate in minutes and seconds of this period judging weights from the instant you picked up the first weight until you judged this last one?"

For the estimates on the succeeding periods, the experimenter said, "Comparing this period with the preceding period to assist you in making your estimate, give me your estimate in minutes and seconds of this last period judging weights. The period began when you lifted the first weight of the series, and ended when you made your last judgment."

The instruction to compare was added because there is some inclincation on the part of subjects making estimates to believe a prior estimate to be too great or too small and therefore to make a correction in a succeeding estimate. It is almost universally true in experiments of this nature that subjects do compare a period with a preceding one to make

[4] Those subjects who had sequence two but who did not make all six estimates, always began the series according to this sequence, and so thirty-four subjects had the order Diff.-Easy to start the series, in contrast with the sequence Easy-Diff. which thirty-three subjects had to start the series.

their estimates. However, unless the individual is instructed to make his estimates proportional, it is possible for two estimates of equal periods to be incomparable, for the above reason.

Results

The comparison of the time-estimates of the periods spent in discriminating relative weights is presented in Table I.

Of the total of 189 pairs of judgments, 127 of the estimates of the period of difficult activity were smaller than the other estimates.

TABLE I

Results of Estimates of Paired Periods Spent in Making Easy and Difficult Discriminations between Weights

Seq. of Trials Number	1st Pair of Judgments		2nd Pair of Judgments		3rd Pair of Judgments		Total	
	D < E	D > E	D < E	D > E	D < E	D > E	D < E	D > E
One	28	5	20	13	24	9	72	27
Two	13	21	25	7	17	7	55	35
Total	41	26	45	20	41	16	127	62

D < E means the period of difficult discriminations was estimated less than the period of easy ones.

D > E means just the reverse of D < E.

This proportion is reliable:[5] $p = .67; \sigma_p = .034; p - 50 = 5\sigma_p$. With the 67 first pairs of judgments which were affected by the factor of position omitted, 86 are less for the difficult periods; 36, greater. This proportion is statistically reliable: $p = .70; \sigma_p = .04; p - 50 = 5\sigma_p$.

With the individual as the unit, by considering that he found time less at the difficult activity if three out of three, two out of three, two out of two, or one out of one of his pairs of estimates were less for the difficult periods, the results are as follows in Table II.

[5] This statistical treatment is based upon the basic formula for the statistics of categories, $\sigma_p = \frac{\sqrt{pq}}{\sqrt{N}}$, where p is one proportion; q, the other; N, the number of cases. (T. L. Kelley, *Statistical Method*, The Macmillan Co., New York, 1923, p. 90.) P is considered reliable when $p - 50 = 3\sigma_p$.

TABLE II

DISTRIBUTION OF INDIVIDUALS ACCORDING TO THE APPEARANCE OF TIME TO THEM AT DIFFICULT AND EASY WEIGHT DISCRIMINATIONS

Those Who Had Sequence	Number of Individuals Who Found D < E	D > E
Number One	27	6
Number Two	25	9
Total	52	15

The proportion of individuals who, on the whole, found time less at the difficult activity is reliable: $p = .78$; $\sigma_p = .051$; $p - 50 = 5.5\sigma_p$.

The last fifteen of the subjects were asked in which of the last two periods they put forth more effort or concentrated more greatly on the task. Eleven reported that they detected the difficult periods, put forth more effort and concentrated their attention more greatly on the activity during that period. Three reported that they lost interest, but regained it toward the last, and put forth more effort in the very last period (an easy one). Two of these last mentioned estimated this easy period less than the preceding difficult one. One of the fifteen reporting stated that she concentrated less on the last difficult period than on the last easy one, stated that she 'got to wondering' if the experimenter were passing out the same weight all the time. She estimated this difficult period greater than the following easy one.

All subjects were asked after the series if they detected the difference in the activities of the contrasting periods, greater differences in weights in some than other periods. Most did. Some did not. These data were not accurately recorded, so that no correlations are possible between the estimates of those who knew the difference and those who did not.

Attention is directed to the results of the first pairs of judgments in Table I. With the factor associated with first position operating in the same direction as the variable of difficulty, the second period was estimated less than the first 28 to 5. With this factor operating in the opposite direction to the variable of difficulty, the second was estimated less

than the first 21 to 13. These facts suggest that between these first two periods, there is a factor associated with position that is of greater influence on time estimation than the variable of degree of difficulty intended as the principal variable. With first and second periods as the categories, second is estimated less 49 times to 18 greater. This proportion is statistically reliable: $\sigma_p = .054$; $p - 50 = 4\sigma_p$.

It is suggested that this first trial at the task may result in a lower degree of unity in the organization of the activity than succeeding trials when the individual becomes more familiar with it and can proceed with dispatch in the activity. It may be suggested, too, that this may be due to a greater urge not as well directed in a first as in subsequent trials.

Conclusion

Both on the basis of paired judgments and on the basis of the individual's tendency to estimate, it has been shown with statistical reliability that individuals judge periods of time spent in making difficult discriminations less than equal periods spent in making easy discriminations to a greater extent than they do the reverse. From the results it is shown, too, that there is some factor associated with the very first periods of this activity that operates toward producing relatively larger estimates of first than second periods.

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GLASS-CAPILLARY INK-WRITING MARKERS FOR USE IN KYMOGRAPH RECORDING

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Although ink-recording tambours, signal markers, time markers, and muscle levers have been available commercially for some time, the use of these instruments in psychological laboratory work has been limited by their cost and by their relative sensitivity as compared to smoked-paper recorders. In an attempt to substitute ink-recording for smoked-paper recording in a general laboratory course in psychology, the writer has devised a glass-capillary ink-writing element which

FIGURE 1

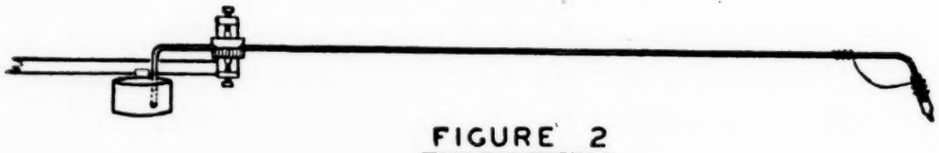

FIGURE 2

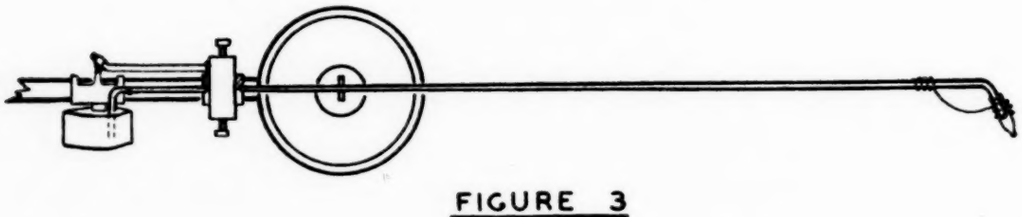

FIGURE 3

can be made sufficiently sensitive for most recording purposes and, in addition, can be adapted to any sort of tambour, time marker, signal marker, or muscle level at a negligible cost.

The writing element (Fig. 1) is constructed of a glass capillary, drawn from 8 mm glass tubing. The two ends of the capillary are bent at the appropriate angles, as shown in the figure. This long capillary is ordinarily drawn to a diameter

of approximately 1 mm. At one end of the capillary is mounted a short piece of somewhat larger glass tubing, the inside diameter of which is large enough to allow easy insertion of the long capillary. This short piece of tubing serves as the writing point. It is made by first drawing a piece of 12 mm tubing to a diameter of about 1.5 mm. A short piece of this is then heated in a fine flame and drawn out to a string. The string is broken off near the body of the capillary, leaving a pointed piece of tubing with a fine capillary at one end. The writing point is finished off either by gently rubbing it on a fine whetstone, or by heating it momentarily in a very low flame. If a heavy line is desired in recording, the point of the capillary can be broken off nearer the body of the piece of tubing. In all of the elements so far constructed by the writer, the sliding element at the end of the long capillary and the angled end of the long capillary have been made approximately 1 cm in length. The writing point is mounted on the long capillary by means of a light silver or copper wire, as shown in the diagram. This wire serves to hold the writing element in place and permits some up-and-down movement of the recorder without much change in pressure on the writing point. The wire is fixed firmly to the glass by means of glue.

The exact specifications for the construction of a writing element of this sort depend upon the type of marker to which it is to be adapted. Such elements may be easily mounted on standard heart levers or light muscle levers [1] for the recording of tremor rate or similar movements. Figure 2 illustrates the mounting of an ink-recorder on a heart lever. The long capillary tubing is drawn to a length of approximately 12 cm, and angled at one end for the fitting of the writing point. The pivot of the instrument is removed from the point bearings, and the wire lever, inserted through the hole in the pivot, removed. This hole is then enlarged by a small drill to a diameter of 1 mm to allow the insertion of the capillary. About 3 to 4 cm of the capillary is allowed to extend through one side of the pivot. The ink reservoir,

[1] Harvard Apparatus Co.

consisting of a small metal cup, approximately 1 cm in diameter, is soldered to the shaft of the lever in the manner shown, about 2 cm away from the bearing of the lever. These cups can be made of small pieces of thin brass tubing or of the metal caps of small medicine bottles. After the short end of the capillary has been bent 90°, the pivot is replaced in its bearings. The recorder is completed when the writing point has been attached to the long capillary. It will be found necessary to remount the bearing on the shaft of some heart levers before the apparatus shown in Fig. 2 can be constructed. The writer has successfully used a lever of this sort in recording directly the eye-wink of cats by attaching a light silk thread to the eyelid of the animal and leading it directly to the writing element, which is kept under light tension by means of a strip of golf-ball rubber.

The writing element may be mounted in a similar way to simple tambours.[2] In this case also, the pivot of the tambour must be removed, the wire lever taken out, and the hole in the pivot redrilled to a diameter of about 1 mm. The writing element is then inserted in the pivot, as described above. The ink-cup is soldered to the slider on which the bearing and pivot are mounted, as shown in Fig. 3. This should be done so that the ink-cup can be moved with the slider when adjustments of the length of the tambour lever are desired. It is also necessary to mount the cup so that it extends approximately 5 mm away from the edge of the slider. The capillary is fixed to the metal rider on the rubber diaphragm of the tambour by means of rubber cement. Ink-writing tambours of this sort will fulfill most of the ordinary requirements of tambour recording which arise in research and in laboratory exercises in the psychological laboratory.

In constructing ink-recording elements for time markers and signal markers, it is possible to dispense with the spring writing point of the recorder. Instead, a writing point is obtained by smoothing the end of the capillary itself. The time marker shown in Fig. 4 is constructed by mounting the magnets of a small electric bell on a length of $\frac{1}{4}$ in. brass tub-

[2] Harvard Apparatus Co.

ing. A piece of piano wire, 1 mm in diameter, is then mounted in a slider, and allowed to rest on a small brass bridge. The bridge and slider are both constructed so that they may be moved along the brass tube. The piece of piano wire, which is about 12 cm in length, is allowed to extend approximately 3 cm beyond the end of the magnets. A glass capillary, 6 to 8 cm in length, drawn to a point and smoothed at the writing end, is mounted on the end of the wire by means of adhesive tape or fine wire windings. At the end of the magnet, directly below the piano wire, is placed the ink-cup, as shown in the diagram. Movements of the wire are damped by means of a piece of adhesive tape attached to the side of the magnet and wrapped around the wire itself. This arrangement of the ink-writing element will provide a silent time marker which can be operated at frequencies far above those ordinarily available commercially.

In Fig. 5 is shown the arrangement of the ink-writing

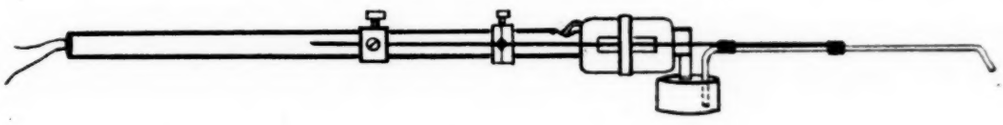

FIGURE 4

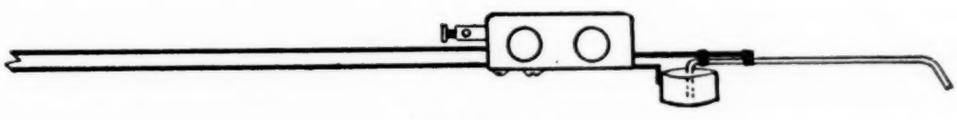

FIGURE 5

element for use with an ordinary signal marker.[3] The lever of the marker has been lengthened 3 cm by soldering a small piece of brass wire to the end of the lever. On this is mounted the ink-writing element, which is drawn to a length of approximately 8 cm and pointed at the end. The ink cup is easily mounted on a strip of brass which is either soldered to

[3] Harvard Apparatus Co.

the base of the marker or screwed to the base by the same screws which hold the shaft of the marker.

In attaching ink-writing elements to the more expensive tambours, the type of mounting used with the time markers may be used, since it is not possible to lead the capillary of the element through the pivots of such tambours.

All of the recorders here described have been used and tested with horizontal movement of the recording paper, but since the markers operate on the basis of capillary action, they can be used as well with vertical movement of the recording paper. Suitable ink for recording purposes can be purchased from various sources, but the writer prefers ink made from 2 percent thionin in distilled water, mixed with an equal amount of glycerin. Other analine dyes may be substituted for thionin. A fairly satisfactory black ink can be secured by adding glycerin to ordinary drawing inks, but this has the disadvantage of sometimes clogging the capillaries when they are not in use.

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